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Essays on the Determinants of Human Capital Investment

Elijah Neilson
Clemson University, elijah.neilson@gmail.com

Essays on the Determinants of Human Capital Investment

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Elijah Neilson
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Accepted by:
Dr. Robert Fleck, Committee Chair
Dr. Peter Blair
Dr. Jorge Garcia
Dr. Devon Gorry

Abstract

The strength of the overall economy depends critically on the human capital – the knowledge and skills – of the workers in that economy. Investment in education is a central way for individuals to acquire human capital. The acquisition of human capital through education can also provide an important signal of individual ability, and can otherwise be rewarded with better labor market outcomes and opportunities. This dissertation explores important determinants of human capital investment by examining key incentives that influence the decision to invest in a college education.

In the first study, I examine the college educational, earnings, and employment responses to local labor demand shocks brought about by recent innovations in horizontal drilling and hydraulic fracturing. I find that a boom in fracking production within a county causes a reduction in college enrollment rates at four-year institutions, an increase in earnings, and an increase in employment for both men and women, with stronger effects for men. The decline in college enrollment during a boom is largely reversed as fracking production slows within a county. Educational attainment, however, remains persistently low for cohorts who experience the biggest enrollment declines. Workers who never attend college experience relatively larger earnings and employment gains, when compared to college-educated workers. These findings reveal that fracking-induced shifts in labor demand raise the opportunity cost of, and reduce the relative returns to, college.

In the second study, I analyze how new fracking production at varying distances from a particular county affects earnings, employment, and college enrollment within that county. I find that new fracking production up to 60 miles away generates positive earnings and employment gains within a county, most notably for non-college-educated men. New fracking production within 40 miles of a county is also associated with reductions in enrollments at two-year and four-year institutions. Beyond a distance of 60 miles, I find little evidence that new fracking production affects earnings, employment, or college enrollment. These findings have important implications

for many counties throughout the United States, even if they do not engage in fracking, but are especially relevant for the top-producing and surrounding counties who account for the vast majority of the expansive increase in fracked oil and gas production over the last two decades.

In the final study, I examine the effect of unilateral divorce laws on college attainment. Exploiting state variation in the adoption of unilateral divorce laws, I show that both women and men are less likely to report having a bachelor's degree in states that adopted unilateral divorce laws. This reduction in human capital investment occurs in states with community property laws, where the law requires an even split of the couple's assets in the event of a divorce and is most pronounced for white women and white men. I find no distortionary effects of unilateral divorce laws on the human capital decisions of black men or black women, even in states with community property laws.

Dedication

To my amazing wife Haley and our wonderful children.

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Chapter 1

The Fracking Boom, Local Labor Market Opportunities, and College Attainment

1.1 Introduction

Models of human capital investment predict that an increase in the earnings of less-educated workers relative to the earnings of more-educated workers will, *ceteris paribus*, reduce investments in schooling. The reason is that the increased earning potential among the less-educated will raise the opportunity cost of schooling and, thus, reduce the returns to education. In this paper, I study this phenomenon empirically, using county-level data to identify the effects of labor demand shocks caused by the fracking boom.

Since the early 2000s, innovations in horizontal drilling and hydraulic fracturing ("fracking") have created oil and natural gas booms in many counties in the United States. Technological change, together with preexisting deposits of oil and natural gas, created large shocks to income and employment in these counties. Between 2005 and 2015, average annual earnings per worker increased by 22.1 percent in fracking boom counties, but by only 4.4 percent in counties from non-fracking states. Over the same period, employment increased by 20.7 and 4.3 percent in fracking boom counties and counties from non-fracking states, respectively.[1] Although the fracking boom provides an ideal setting to study the college educational response to local earnings and employment shocks, it has received little attention in the literature.

An important feature of the boom is that fracking production expanded and contracted over a relatively short time period in many counties. Because people can go back to school later in life, sharp changes in short-run economic conditions might not have an effect on ultimate educational choices (Card and Lemieux, 2001). Temporary shocks to income could even provide a source of financing that leads to higher levels of educational attainment, especially for students facing considerable financial constraints on educational investment.

To estimate the short and long run effects of fracking-induced labor demand shocks on college investment, earnings, and employment, I first use a comprehensive data set of oil and natural gas production from all wells drilled in the United States to identify which oil and gas producing counties experienced a boom in fracking production, and in what year the boom began in each county. For each boom county, I then construct a synthetic control of counties from non-fracking states that most resemble the boom county based on pre-boom characteristics. The period-specific effects of fracking estimated using the synthetic control method offer a way to examine how the educational and labor market outcomes evolve over time as fracking production expands and contracts within a boom county. Averaging these dynamic effects over time provides a measure of the overall average effects of fracking.

Over the course of a boom cycle, college enrollment is lower on average in fracking boom counties relative to their synthetic controls. There is no significant difference, however, in graduation rates between the two sets of counties. Using data from the American Community Survey (ACS) and the Integrated Postsecondary Education Data System (IPEDS), I find specifically that the proportion of young men enrolled in college is about 4.7 percentage points smaller on average in fracking boom counties relative to the proportion enrolled in their respective synthetic control counties; a reduction of 12.5 percent relative to the mean proportion enrolled of 37.6 percent. Similarly, the proportion of young women enrolled is about 3.9 percentage points smaller, a reduction of 8.7 percent relative to the mean proportion enrolled of 44.6 percent. The reduction in enrollment

[1] Statistics are derived from employment count and average annual earning measures from the Quarterly Workforce Indicators. Feyrer et al. (2017) document that fracking resulted in increased earnings and employment in the oil and natural gas industry as well as non-oil and natural gas industries. They find that within a county, each million dollars of new fracking production produces $80,000 in wage income and $132,000 of royalty and business income. They also claim that on aggregate, U.S. employment increased by as many as 640,000 as a result of fracking.

is driven by reductions in enrollment rates at four-year institutions. Indeed, I find a contrasting result for two-year enrollment rates. If anything, they may have increased in fracking boom counties, particularly for women.

The observed effects of fracking-induced labor demand shocks on college attainment are consistent with traditional models of human capital investment, including the model I present in Section 1.3, because fracking causes a large proportional increase in the earnings of non-college-educated workers. Panel (a) of Figure 1.1 shows the average value of fracked oil and gas production in boom counties five years prior to, and five years following, the start of a boom. Panel (b) shows the corresponding trends in the natural log of average annual earnings in boom counties and their synthetic controls over these same years, by educational attainment. Prior to the boom, earnings in boom counties and their synthetic controls are identical. As fracking production increases, earnings in boom counties increase for both non-college and college-educated workers relative to their synthetic controls, with the effects being proportionately larger for non-college-educated workers. Using data from the Quarterly Workforce Indicators (QWI), I find that the average annual earnings of non-college-educated men in boom counties are about $5,467 (16.2 percent) larger than those in their synthetic control counties during the ten year period following the start of a boom, while average annual earnings of college-educated men are about $7,045 (11.3 percent) larger. Average annual earnings are about $1,985 (9.4 percent) and $2,034 (5.3 percent) larger for non-college-educated and college-educated women, respectively. The college premium in boom counties, as measured by the *ratio* of college-educated worker earnings to non-college-educated worker earnings, decreases by 0.12 (6.7 percent) for men and 0.04 (1.9 percent) for women relative to the college premiums in their synthetic control counties. I also find positive effects of fracking on the employment-to-population ratio in boom counties, with the employment effect being relatively larger for non-college-educated workers than college-educated workers.

One benefit of using the synthetic control method in this study is the ability to capture the dynamic effects of a fracking boom as fracking production expands and contracts in a county. The dynamic effects of fracking on labor market outcomes and fracking production following the start of a boom are positively correlated. Thus, the changes in earnings and employment are largest in magnitude when fracking production is at its peak, about five years following the start of a boom. This positive correlation is most pronounced for the employment effects, and appears regardless of gender or educational attainment.

The dynamic effects of fracking on college enrollment and fracking production following the start of a boom are negatively correlated. This implies that decreases in college enrollment are largest in magnitude during the peak of the boom, but any effect tends to be nullified after fracking production slows down in a boom county. Although the decline in college enrollment is generally reversed as fracking production slows in a boom county, I find that college enrollment and attainment remains persistently low for particular cohorts. Specifically, I find that individuals aged 16 to 19 at the start of a boom are less likely to be enrolled throughout the duration of the boom, and no more likely to be enrolled after the boom. Moreover, I find that 10 years following the start of a boom, individuals in these cohorts living in boom counties have less educational attainment than those of the same cohorts in the synthetic control counties, despite having similar levels of attainment prior to, and at the start of, the boom. These results suggest that reduced educational attainment is an enduring effect of the fracking booms, despite the transitory nature of the booms.

The average and period-specific effects of fracking on earnings and employment are consistently larger in magnitude for men than for women. If an increase in the opportunity cost of college attendance and a corresponding decrease in the relative returns to college are important mechanisms through which fracking affected college investment, then a relatively larger effect on male college enrollment rates is also expected. I find that on average, the effect of fracking on college enrollment rates is relatively larger for men than for women, with this disparity being particularly pronounced during the peak years of a boom.

The model I present in Section 1.3 explores other potential channels through which fracking may affect college investment. Increases in tax revenues in a boom county that get channelled into school spending, for example, might alter teacher quality and student productivity, as well as provide more opportunity for scholarship funding. Additionally, college educational attainment may be influenced by rising parental income in boom counties (Blanden and Gregg, 2004). With these factors considered, the net effect on educational attainment is theoretically ambiguous. My empirical evidence, however, supports the conclusion that an increase in the opportunity cost of college is the dominant channel through which booms in fracking production affected college educational attainment.

Although labor market opportunities resulted in migration into these boom counties, particularly in North Dakota and the Bakken region (Wilson, 2016), changes in the composition of the population are not driving my results. The estimated effects of fracking on college enrollment rates remain robust to restricting the ACS sample to include only those who had not changed their residence since prior to any boom in fracking production.

In the next section I provide a brief literature review on fracking and other economic booms. In section 1.3, I present a theoretical model of human capital investment. In Section 1.4, I offer a brief background on fracking production, discuss how I define booms and identify boom counties, and outline the various data sources used in this study. Section 1.5 explains my empirical strategy and method for conducting inference. In Section 1.6, I present results. Section 1.7 concludes.

1.2 Literature Review

My work is most directly related to the growing literature on the effects of the fracking boom. Scholars have looked at the effect of the fracking boom on a variety of outcomes, to which I add an examination of the long-term effects on college enrollment and graduation.[2]

Researchers have shown consistently that the earnings and employment effects of the fracking boom are not only substantial, but extend beyond the mining industry to construction, transportation, and other industries dominated by less-educated men (Feyrer et al., 2017; Bartik et al., 2019). Consequently, fracking-induced labor demand shocks have largely favored men without a college degree (Kearney and Wilson, 2018; Cascio and Narayan, 2017). My findings on the local labor market effects of the fracking boom are consistent with other findings in the literature.

Despite several studies regarding the relationship between historic resource extraction and human capital investment, estimates of the educational effects of resources are scarce and inconclusive (Marchand and Weber, 2018).[3] Even less attention has been paid to the effects of the recent fracking boom on educational attainment, with several notable exceptions. Cascio and Narayan (2017), Rickman et al. (2017), and Niekamp (2019) find that in general, fracking had a negative effect on educational outcomes. Specifically, fracking increased high school dropout rates of male teens, both overall and relative to females throughout the U.S. (Cascio and Narayan, 2017), though

[2]Outcomes shown to have been possibly affected by the fracking boom include income and employment (Weber, 2014; Bartik et al., 2019; Feyrer et al., 2017; Krupnick and Echarte, 2017; Maniloff and Mastromonaco, 2017), high school attainment (Cascio and Narayan, 2017; Rickman et al., 2017; Niekamp, 2019), migration (Wilson, 2016, 2017), crime (James and Smith, 2017; Street, 2018; Andrews and Deza, 2018), school finance, teacher quality and student achievement (Marchand et al., 2015), as well as marriage, divorce, and birth rates (Kearney and Wilson, 2018).

[3]For studies regarding the relationship between historic resource extraction and human capital investment, see for example Black et al. (2005), Papyrakis and Gerlagh (2007), Michaels (2011), Emery et al. (2012), Haggerty et al. (2014), Morissette et al. (2015), Douglas and Walker (2017), and Kumar (2017).

perhaps not in North Dakota (Niekamp, 2019). In North Dakota, enrollment rates at four-year colleges decreased significantly in core-oil producing counties relative to non-oil counties (Niekamp, 2019). There is evidence that high school and college attainment decreased in Montana and West virginia as well (Rickman et al., 2017).

Considering the existing literature as a whole, this paper provides three key contributions. First, I document the dynamic effects of the fracking boom over a relatively long period of time. Importantly, this allows me to see what happens to labor market and educational outcomes within a county not only while fracking production is booming, but also when production slows. Second, I track college enrollment and attainment of the cohorts most likely to be affected by the boom as they age. This allows me to analyze whether individuals are simply delaying their college going, or foregoing it altogether. Third, this paper analyzes the effect of the fracking boom on college educational outcomes across the entirety of the United States. Previous research focuses almost purely on high school attainment, and the few studies that analyze college attainment focus on a particular state or subset of states.

1.3 Theoretical Model

To guide my empirical analysis, I model the essential factors linking fracking to educational attainment. My approach draws heavily on existing models of human capital investment (Becker, 1964; Cascio and Narayan, 2017; Charles et al., 2018). Following Charles et al. (2018), individuals will invest in human capital until the marginal benefit equals the marginal cost. The marginal benefit consists of the expected lifetime earnings associated with the investment. The marginal cost consists of direct costs of education, such as tuition, books, and fees; indirect, or "psychic" costs of college enrollment, relative to those of working; and the implicit opportunity cost, or foregone earnings as a result of going to college instead of working. Fracking can affect college enrollment decisions through any one of these channels, and the purpose of this model and empirical work to follow is to not only understand whether it has an overall effect, but the channels through which that effect arises.

The individuals in my framework are young adults, who are aged a_t in year t and live until age T. These young adults decide whether to immediately participate in the labor market, or attend college before participating in the labor market. Students differ in their academic ability θ_i, distributed according to a Uniform distribution over [0,1]. Let Z denote the direct costs of college, and b the interest rate at which students can borrow. College students incur indirect or psychic costs $z(\theta_i) = \psi(1-\theta_i)$ from attending college.[4] Labor market participants with and without college training receive labor market incomes ω_t^c and ω_t^{nc} in year t, respectively.

Given the model setup above, then with current information Λ_i, the value of not going to college, $V_{it}^{nc}(\theta_i)$, is simply the discounted present value of expected lifetime earnings without college training

$$V_{it}^{nc}(\theta_i) = \sum_{k=0}^{T-a_t} \frac{1}{(1+r)^k} E\left[\omega_{t+k}^{nc} | \Lambda_i\right], \tag{1.1}$$

and the value of going to college, $V_{it}^{c}(\theta_i)$, is the discounted present value of lifetime earnings with a college degree, net the direct and indirect costs

$$V_{it}^{c}(\theta_i) = \sum_{k=1}^{T-a_t} \frac{1}{(1+r)^k} E\left[\omega_{t+k}^{c} | \Lambda_i\right] - (1+b)Z - \psi(1-\theta_i). \tag{1.2}$$

Letting $\pi_k^c = \omega_k^c - \omega_k^{nc}$ denote the college income premium in some year k, then the expected lifetime premium that a person of ability θ_i gets from attending college in year t, $R_{it}^c(\theta_i)$, is

$$R_{it}^c(\theta_i) = V_{it}^{c}(\theta_i) - V_{it}^{nc}(\theta_i) = \sum_{k=1}^{T-a_t} \frac{1}{(1+r)^k} E\left[\pi_{t+k}^{c} | \Lambda_i\right] - (1+b)Z - \psi(1-\theta_i) - \omega_t^{nc}. \tag{1.3}$$

The first term in equation (1.3) is individual i's discounted present value of the expected future lifetime college income premium at time t, or the sum of their expectations of the college income premium for every year of their future working life, given their current information Λ_i. The middle two terms are the direct and indirect costs of going to college, and the last term is the labor market earnings foregone in year t as a result of enrolling in college and not working, or the opportunity cost of going to college. An individual with ability θ_i will go to college if their expected lifetime premium from attending college in year t is greater than zero. In other words, when $V_{it}^{c}(\theta_i) > V_{it}^{nc}(\theta_i)$.

Figure 1.2 provides a graphical representation of this equilibrium. θ^* is the threshold ability level such that individuals with $\theta_i < \theta^*$ will not go to college, individuals with $\theta_i > \theta^*$ will go to college, and individuals with $\theta_i = \theta^*$ are indifferent between college or not.

Equation (1.3) formalizes the main channels through which fracking can influence the decision to attend college. Fracking may have resulted in an immediate increase in the local labor

[4]The linear functional form of z is imposed for graphical simplicity only.

market income of non-college graduates, thus increasing the opportunity cost of going to college. As can be seen in Figure 1.3, this would increase the threshold ability level to θ', resulting in fewer college attendees. Furthermore, the technological innovation resulting in the fracking boom may have had enduring effects on the labor market incomes of both college graduates and non-college graduates. If fracking increased the expected lifetime labor market income of non-graduates relatively more than for graduates, then the college income premium would decrease, further increasing the threshold ability level to θ'', resulting in even fewer college attendees.[5] The fracking boom could have also affected the indirect costs of going to college. For example, fracking may have resulted in increased resources in the homes and high schools of prospective college students, perhaps decreasing the psychological costs associated with attending school. If this is relatively more beneficial for low ability individuals than high ability individuals, the upward sloping line will shift up and become less steep, resulting in a threshold ability level θ'''.[6]

A well identified reduced-form estimate of the effect of fracking captures the college educational response to all of these channels. In other words, the net college enrollment effect for the average area with fracking would be $\theta''' - \theta^*$. The reduced form effect can be broken up into the three channels described above,

$$\theta^{RF} = \theta''' - \theta^* = (\theta''' - \theta'') + (\theta'' - \theta') + (\theta' - \theta^*), \tag{1.4}$$

where $(\theta''' - \theta'')$ is the indirect cost channel, $(\theta'' - \theta')$ the college income premium channel, and $(\theta' - \theta^*)$ the opportunity cost channel.

[5]These points can be more formally illustrated by considering a simple constant elasticity of substitution production function for aggregate output Q with two types of labor inputs, college-educated (c), and non-college-educated (nc),

$$Q = \left[\alpha(A_c L_c)^{\frac{\sigma-1}{\sigma}} + (1-\alpha)(A_{nc} L_{nc})^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}},$$

where L_c and L_{nc} are the quantities employed of college-educated and non-college-educated workers, A_c and A_{nc} represent college and non-college educated labor-augmenting technological change, α is a technology parameter indexing the share of work activities allocated to college-educated workers, and σ is the elasticity of substitution between college and non-college-educated labor. Under the assumption that workers are paid their marginal products,

$$ln\left(\frac{\omega^c}{\omega^{nc}}\right) = ln\left(\frac{\alpha}{1-\alpha}\right) + \left(\frac{\sigma-1}{\sigma}\right) ln\left(\frac{A_c}{A_{nc}}\right) - \frac{1}{\sigma}\left(\frac{L_c}{L_{nc}}\right).$$

Thus if $\sigma > 1$ and technological innovations in fracking acted to allocate a larger share of work activities to non-college-educated workers (a decrease in α), or otherwise resulted in a relative increase in non-college-educated worker productivity (a decrease in $\frac{A_c}{A_{nc}}$), then this would result in a decrease in the college premium.

[6]By reducing student-to-teacher ratios, increasing teacher salaries, making school years longer, or altering any other school input, increases in school spending can lead to higher educational attainment (Jackson et al., 2015). Marchand et al. (2015) find however, that school districts in Texas responded to the fracking-caused tax base expansion by spending more on capital projects, but not on teachers. As a result, fracking led to high teacher turnover and more inexperienced teachers, ultimately leading to slightly lower student achievement in these districts.

While the reduced form effect is interesting and important, distinguishing among these channels is key to understanding the causal relationship between fracking and college attainment. There is a problem however, in attempting to identify multiple channels with one exogenous shock – the introduction of fracking. Suppose there are two distinct groups in the population, group f and group m, and that group f does not experience the college premium and opportunity cost effects ($\theta''_f - \theta'_f = 0$ and $\theta'_f - \theta^*_f = 0$) but group m does. Suppose further that the indirect cost effect is the same for the two groups ($\theta'''_m - \theta''_m = \theta'''_f - \theta''_f$). Under these conditions, differencing the reduced form effects across these two groups will identify the effects of fracking working through the two labor market channels,

$$\theta^{RF}_m - \theta^{RF}_f = (\theta''_m - \theta'_m) + (\theta'_m - \theta^*_m). \tag{1.5}$$

In the empirical strategy to follow, I will incorporate this idea by verifying that fracking has larger impacts on the expected earnings of men than women, and by estimating the difference in effects of fracking on educational outcomes of men and women. Hence, empirically, males represent group m and females represent group f.

1.4 Context and Data

1.4.1 Fracking Production

Shale rock formations far below the surface of the earth hold enormous deposits of natural gas and oil (often referred to as shale gas and shale oil). Hydraulic fracturing is a well stimulation technique involving high-pressure injection of fracking fluid, primarily consisting of water, sand, and other thickening agents, to create and maintain fractures in the shale rock allowing the shale oil and gas to be released. In a review of the economic, policy, and technology history of shale gas development, Wang and Krupnick (2015) suggest that a number of factors converged in the early 2000s that made it profitable for firms to produce large quantities of shale gas, but that the most important factor was innovations in technology.[7] The review of Wang and Krupnick (2015) finds that some of the key innovations in fracking technology came from government research and development and private entrepreneurship aimed at developing unconventional natural gas (for

[7] Some other factors suggested include high natural gas and oil prices in the 2000s (see Figure 1.5), government policy, private entrepreneurship, private land and mineral rights ownership, market structure, favorable geology, water availability, and natural gas pipeline infrastructure.

example, shale gas), and some of the innovations in fracking technology (for instance, horizontal drilling and three-dimensional seismic imaging) came from the oil industry where firms sought to explore and produce unconventional oil instead of unconventional gas.

These technological innovations made it more cost-effective to produce shale oil and gas, and as a result the share of natural gas and oil production coming from shale resources has increased dramatically since the early 2000s. Figure 2.2 shows the aggregate annual level of oil and gas production by drill type of wells that first started producing oil and gas in the year 2000 or later. Production from traditional vertically drilled wells exceeded that of horizontally drilled (fracked) wells in the early 2000s and remained relatively constant between 2000 and 2017. Starting between 2006 and 2010 however, shale oil and gas production increased tremendously across the U.S., with horizontally drilled production far surpassing production from traditional vertically drilled wells.

Data on local level oil and gas production come from DrillingInfo, a private firm that collects lease, permit, and production data on all wells drilled in the United States.[8] The data indicate drill date, monthly production amount, drilling direction (vertical or non-vertical), and county.[9] The sample consists of monthly production of oil, measured in barrels, and gas, measured in thousands of cubic feet (MCF), on properties that began producing either oil, gas, or oil and gas at some point after January 1st, 2000.

To convert oil and gas production into comparable dollar amounts, I use average annual national prices for oil and gas, recorded by the Energy Information Administration (EIA), and create a measure of the value of fracked oil and gas produced.[10] I then use the Consumer Price Index to adjust all dollar amounts to 2010 dollars.

For consistency across samples, I aggregate all oil and gas production data to the county-year level. To take into account the relative size of a county, I use the annual intercensal county resident population estimates from the U.S. Census Bureau to create a per capita measure of the total value of oil and gas production. Specifically, I define $Production_{cy}$ to be the total value of

[8]The use of these data were provided by DrillingInfo through an academic use agreement.

[9]Similar to Feyrer et al. (2017) and Kearney and Wilson (2018), I consider oil and gas produced from non-vertical wells as fracked oil and gas.

[10]For natural gas, I use the reported average annual citygate prices, which represent the total cost paid by gas distribution companies for gas received at the point where the gas is physically transferred from a pipeline company or transmission system. This price is intended to reflect all charges for the acquisition, storage, and transportation of gas as well as other charges associated with the local distribution company's obtaining the gas for sale to consumers. For crude oil, I use the reported West Texas Intermediate (WTI) average annual price. Prices of WTI are often listed in oil price reports, alongside other important oil markers, like UK Brent or the OPEC basket. WTI crude oil is also the underlying commodity of the Chicago Mercantile Exchange's oil futures contracts. The price of other crude oils, such as UK Brent crude oil, the OPEC crude oil basket, and Dubai Fateh oil, can be compared to that of WTI crude oil.

fracked oil and gas production per capita in county c in year y. Figure 1.6 shows the total value of fracked oil and gas production by county between 2000 and 2017. In all, there were 745 counties in 27 states where at least some shale oil and gas production occurred between 2000 and 2017. Fracking production was most heavily concentrated in Texas, North Dakota, Oklahoma, Louisiana, Pennsylvania, and Wyoming.

Averaging $Production_{cy}$ over 2000 to 2017, the average county's value of fracked oil and gas per capita was $\$9,150$, while the median was only $220. Figure 1.7 shows the average annual value of fracked oil and gas production over time by quintile of this distribution. Although fracking occurred in almost 750 counties over the sample period, the vast majority of fracked oil and gas came from a relatively small number of counties. The highly skewed distribution of fracking production highlights the importance of identifying and analyzing the effects of fracking in these select few counties that experienced a boom.

1.4.2 Boom County Identification

Consider the statistical model

$$Production_{cy} = \lambda_c + \phi_y + \varepsilon_{cy} \,, \tag{1.6}$$

where $Production_{cy}$ is the total value of fracked oil and gas production per capita in county c in year y, λ_c represents county fixed effects, and ϕ_y represents year fixed effects. I use least-square residual variation in $Production_{cy}$, which nets out county and year fixed effects, to identify boom counties. I consider a county a boom county if its residual fracking production is consistently large relative to other counties over the sample period. Specifically, I identify the 75th percentile in the distribution of residual fracking production for each year. If a county is in the upper quartile of these distributions in the majority of years in the sample, it is considered a boom county. 108 of the 745 counties with fracking production were considered boom counties.

The timing of a boom in a boom county varied according to when fracking production began in each individual county. Thus, when analyzing the average or aggregate effects of fracking on educational and labor market outcomes, it is important to do so in event time, where the event is defined to be the year in which fracking production saw a marked increase in each county. Figure 1.8 gives four examples of counties experiencing booms in different years. The vertical lines represent the event year in each county. In each case, fracking production is relatively stable prior to the event year, but then continues to increase for about five years. This pattern was fairly consistent across boom counties. Indeed, the event year for each county was identified as the year in the sample in which fracking increased the most over the following five year period. Table 1.1 shows the event year for each of the 108 boom counties in the sample. Figure 1.9 shows average annual standardized residual fracking production in boom counties and non-boom counties, confirming that I have identified counties which in fact experienced a boom, as well as their respective event timing.

1.4.3 Educational Outcomes

My first source of information on educational outcomes comes from the 2005 to 2017 American Community Survey (ACS) samples. Public Use Microdata Areas (PUMAs) are the most detailed geographic areas available in the ACS Public Use Microdata Samples, and are defined as a group of counties, or tracts within counties, with at least 100,000 people. PUMA boundaries do not overlap and are completely contained within states. I do not use ACS samples prior to 2005 because PUMA codes are not available in these samples. For consistency across data sets, I use a PUMA-to-county crosswalk to convert PUMA-year educational measures from the ACS to the county-year level.

Besides current PUMA and state of residence, the ACS contains individual level data on educational attainment, current enrollment status, state of birth, and migration information. To best capture the effect of fracking induced labor demand shocks on educational outcomes, I restrict the ACS sample to individuals aged 18 to 26, the ages in which educational decisions are most often made. I define $Prop.\ Enrolled_{cy}$ to be the proportion of individuals aged 18 to 26 in county c in year y currently enrolled in school. Similarly, I define $Prop.\ Graduated_{cy}$ to be the proportion with four or more years of completed college.

My second source of information on educational attainment is the Integrated Postsecondary Education Data System (IPEDS). IPEDS is a system of interrelated survey components conducted annually by the National Center for Education Statistics. IPEDS gathers information from every college, university, and technical/vocational institution that participates in the federal student financial aid programs in the United States. Included in this rich administrative data is a measure of first-time, full-year enrollments, as well as total graduations, allowing me to identify the number of individuals enrolling for the first time, or graduating, during the booms. I match colleges and universities to counties, and compute gender and county-specific estimates of first-time, full-year enrollments, as well as graduations in each year between 2000 and 2016. I then adjust these first-time enrollment and graduation totals by the size of the population aged 20 to 25 in each county in order to capture per capita first-time enrollment and graduation rates.[11]

One of the advantages of using the ACS is the ability to test to what extent changes in the composition of the population, notably from migration, affect the observed effects of fracking. This is possible by first restricting the sample to individuals aged 18 to 26, ages during which most human capital investments are made. Second, the ACS contains information on the migration activity of the respondents. By restricting the sample to those who have been living in their current residence since prior to any boom, I can assess whether fracking altered the college investment decisions of long-term residents, or if the effects using the unrestricted sample simply reflect changes in the composition of the population.

One drawback of the ACS is the inability to use samples prior to 2005 due to the lack of geographic information of the respondents. Another concern is that ACS data do not distinguish between the type of college a respondent attended or is attending. For example, if an individual reports being enrolled in school, it is unreported whether they are enrolled in a two-year or a four-year college. In addition to data availability before 2005, the IPEDS reports enrollment and graduation totals for different types of colleges separately. Together, the ACS and IPEDS data provide a thorough picture of activity related to college educational attainment before, during, and after county-specific booms in fracking production.

1.4.4 Earnings and Employment

Data on earnings and employment come from the Quarterly Workforce Indicators (QWI). The source data for the QWI is the Longitudinal Employer-Household Dynamics (LEHD) linked employer-employee microdata, covering over 95% of U.S. private sector jobs. The QWI provide local labor market statistics at the county level by industry and worker demographics, such as worker age, gender, educational attainment, and race/ethnicity. These data however, can only be tabulated for two-way groups (for example, by gender and educational attainment of workers). Therefore, for each industry in a county, I have measures of average annual earnings and annual employment

[11] The county population estimates were already grouped according to age by the U.S. Census Bureau.

counts by gender and educational attainment of workers. I define $Average\ Annual\ Earnings_{cy}$ as the average annual earnings in county c in year y (in 2010 \$), and $Jobs/Population_{cy}$ as the total jobs-to-population ratio in county c in year y.

1.5 Empirical Strategy

My empirical work relies on the ability to estimate what would have happened in counties exposed to a boom in fracking had they not experienced the boom. One possible comparison is between boom counties and other fracking counties that are not considered boom counties. Because boom and non-boom counties were both exposed to fracking, they are likely similar along many dimensions. However, it is precisely because fracking occurs in varying degrees in non-boom counties that I elect not to use them as part of a potential control group. To best estimate the effects of fracking on educational and labor market outcomes, I include in my potential control group all counties from states that did not have any fracking production over the sample period. Feyrer et al. (2017) show that the wage and employment effects of fracking production are significant up to 100 miles from where the actual fracking production takes place. By excluding non-fracking counties within fracking states, I limit the possibility of potential control counties experiencing the effects of the boom in a neighboring county within the same state.

Table 1.2 contains descriptive statistics for the main outcome variables from each data set, as well as population characteristics, for fracking boom counties and non-fracking counties. Although the population characteristics are strikingly similar between the boom counties and non-fracking counties, the outcome variables appear to be quite different, especially the educational outcomes. There could, of course, be a concern for estimation if the outcomes in boom counties were on different trajectories than those in non-fracking counties prior to their respective booms. To address this concern, I use the synthetic control method formally introduced by Abadie and Gardeazabal (2003) and Abadie et al. (2010).

The synthetic control method is often used to evaluate the effects of an intervention in comparative case studies. It is a data-driven approach to constructing a weighted average of untreated units that act as a control, to which the treated unit is compared. In the context of this study, the boom counties identified previously are the treated units, and the counties from non-fracking states make up the pool of potential controls for each boom county. The intervention is the boom in fracking production, which I specify as occurring in the county-specific event years described above for each boom county. An additional benefit of using the synthetic control method is the ability to capture the dynamic effects of fracking on each educational and labor market outcome as fracking production expands and contracts in a county.

To outline my empirical strategy, I will begin by providing some notation to evaluate the effect of the fracking boom in a single county. Then I will discuss how I aggregate the county-specific effects into an average effect, as well as my method for conducting inference.

1.5.1 Synthetic Control Method - One Treated Unit

Following the notation of Abadie et al. (2010), I observe $J+1$ counties. Without loss of generality, let the first county be the one to experience a fracking boom, so that there are J remaining counties that serve as potential controls. Let Y_{it}^N denote the educational or labor market outcome that would be observed for county i at time t in the absence of fracking, for all counties $i = 1, \ldots, J+1$, and time periods $t = 1, \ldots, T$. Suppose the intervention occurs in period T_0, then $T_0 - 1$ is the number of periods before the start of the boom (the length of the pre-intervention period), with $1 \leq T_0 - 1 \leq T$. Let Y_{it}^I be the educational or labor market outcome that would be observed for county i at time t if county i is exposed to a boom from period T_0 to T. I assume that fracking had no effect on educational or labor market outcomes in the pre-intervention period, so for $t \in \{1, \ldots, T_0 - 1\}$ and all $i \in \{1, \ldots, N\}$, $Y_{it}^I = Y_{it}^N$.

Let $\alpha_{it} = Y_{it}^I - Y_{it}^N$ denote the effect of fracking for county i at time t if county i is exposed to fracking in periods $T_0, T_0+1, T_0+2, \ldots, T$. Note that this effect is allowed to potentially vary over time. Therefore,

$$Y_{it}^I = Y_{it}^N + \alpha_{it}, \tag{1.7}$$

and the observed educational or labor market outcome for county i at time t is

$$Y_{it} = Y_{it}^N + \alpha_{it} D_{it}, \tag{1.8}$$

where D_{it} is an indicator variable taking on the value of 1 if county i is exposed to a fracking boom at time t and 0 otherwise.

The parameters of interest are $\alpha_{1,T_0}, \alpha_{1,T_0+1}, \ldots, \alpha_{1,T}$, which are the post-intervention period-specific effects of fracking on the educational or labor market outcome of interest. For $t \geq T_0$,

$$\alpha_{1t} = Y_{1t}^I - Y_{1t}^N = Y_{1t} - Y_{1t}^N. \tag{1.9}$$

Note that Y_{1t} is observed. Therefore, to estimate α_{1t}, it is only necessary to come up with an estimate for Y_{1t}^N. Abadie et al. (2010) suggest using

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt} \tag{1.10}$$

for $t \in \{T_0, T_0 + 1, \ldots, T\}$ as an estimator for α_{1t}, where w_j^* is the weight given to potential control county j. The vector of weights $\mathbf{W} = (w_2, \ldots, w_{J+1})$ where $w_j \geq 0$ for $j = 2, \ldots, J+1$ and $w_2 + w_3 + \cdots + w_{J+1} = 1$ is chosen to provide a linear combination of potential control counties that best match the treated county based on pre-intervention values of the outcome variable, as well as other pre-intervention characteristics. The pre-intervention variables I use to identify the synthetic control for each treated county include the value of the outcome variable in each of the pre-intervention years (Abadie et al., 2010), as well as county-year demographic characteristics including the proportion of males, gender-specific proportions of white individuals, and gender-specific shares of individuals aged 20 to 34, 35 to 49, 50 to 64, and over 65.

To capture the average effect of the fracking boom in the treated county, denoted by $\bar{\alpha}_1$, I average the period-specific estimates of fracking over the entire treatment period. Thus,

$$\bar{\alpha}_1 = \left(\frac{1}{T - T_0}\right) \sum_{t=T_0}^{T} \hat{\alpha}_{1t} \,. \tag{1.11}$$

1.5.2 Synthetic Control Method - Multiple Treated Units

Suppose there are G counties that experience a fracking boom in potentially different years. Similar to Cavallo et al. (2013) and Dube and Zipperer (2015), for each treated county $g \in \{1, \ldots, G\}$, I follow the same strategy outlined above to estimate period-specific effects of fracking, denoted by $\hat{\alpha}_{g,l}$, where l represents the number of years since the start of the boom.[12] The estimated average

[12] Combining estimated effects using event time is important in this study because the event year varies across the G treated counties.

period-specific effects then, are given by

$$\bar{\alpha}_l = \frac{1}{G} \sum_{g=1}^{G} \hat{\alpha}_{g,l}. \tag{1.12}$$

With multiple treated counties, I estimate the overall average effect of the fracking boom in the treated counties, denoted by $\bar{\alpha}$, by averaging the period-specific average estimates of fracking over the entire treatment period (Dube and Zipperer, 2015). Therefore,

$$\bar{\alpha} = \left(\frac{1}{T - T_0} \right) \sum_{l=T_0}^{T} \bar{\alpha}_l \, . \tag{1.13}$$

1.5.3 Inference

Large sample inferential techniques are not well suited for comparative case studies such as this, since the number of control counties that receive positive weight and periods in the sample are relatively small. Following Abadie et al. (2010), Dube and Zipperer (2015), and Cavallo et al. (2013), I use exact inferential techniques, similar to permutation tests, to conduct statistical inference. This involves applying the synthetic control method to each of the control counties, finding a large number of average placebo effects, and then examining at each period if the effect of fracking in a treated county is large relative to the distribution of average placebo effects. Specifically, I conduct inference for each period-specific effect, $\bar{\alpha}_l$, by computing a $(1-k)$ percent confidence interval according to the following steps.

1. For each period l, I compute placebo estimates, $\hat{\alpha}^{PL}_{j,l}$, for all potential control counties $j \in \{2, \ldots, J+1\}$.

2. Let $N_{\overline{PL}}$ denote the number of average placebo effects of size G, each combination indexed by $c \in \{1, \ldots, N_{\overline{PL}}\}$. For each period l, I compute an average placebo effect of size G, denoted by $\overline{\alpha}_l^{PL(c)}$, by choosing at random G values of the J placebo estimates, $\hat{\alpha}^{PL}_{j,l}$, and averaging over these estimates. This procedure is repeated with replacement $N_{\overline{PL}}$ times.

3. Let $P_l^{TR} = F^{PL}(\bar{\alpha}_l)$ denote the percentile rank statistic of the period-specific treatment effect $\bar{\alpha}_l$, where F^{PL} is the empirical cumulative distribution function of the $N_{\overline{PL}}$ average placebo effects, $\overline{\alpha}_l^{PL(c)}$. Let $P^{PL}_{l,(p)} = F^{PL}\left(\bar{\alpha}^{PL(c)}_{l,(p)}\right)$ denote the percentile rank statistic of the average placebo effect $\bar{\alpha}^{PL(c)}_{l,(p)}$, which is the p^{th} percentile average placebo effect. Inverting the per-

centile rank test, the $(1-k)$ percent confidence interval of the period-specific treatment effect $\bar{\alpha}_l$ is given by

$$\left(\bar{\alpha}_l - \bar{\alpha}^{PL(c)}_{l,(1-\frac{k}{2})}, \bar{\alpha}_l + \bar{\alpha}^{PL(c)}_{l,(\frac{k}{2})}\right). \tag{1.14}$$

In step 1 above, I compute period-specific placebo estimates for each of the J untreated counties following the same procedure outlined above for the G actual treated counties. This involves considering each control county as "treated," and finding a synthetic control using the remaining $J-1$ control counties. Because the event year varies across the G treated counties, I randomly assign the fraction of control counties to each event year that corresponds to the fraction of the actual treated counties in each event year. Because $\bar{\alpha}_l$ is an average of the G treated counties in each period, step 2 involves computing $N_{\overline{PL}}$ average placebo effects of that same size. I compute $N_{\overline{PL}} = 1,000$ average placebo effects of size G, to which I compare the actual estimated treatment effect. Steps 1 and 2 ensure that the average period-specific placebo effects were found in an identical way as the actual period-specific effects, allowing for meaningful comparisons between the two.

Similar to the procedure taken by Dube and Zipperer (2015), Step 3 involves examining if the average period-specific treatment effect is large relative to the distribution of average period-specific placebo effects. Since the percentile rank statistic is approximately uniformly distributed, I determine whether the percentile rank of the period-specific treatment effect P^{TR}_l lies within the tails of the uniform distribution. Given a statistical significance level of k percent, I cannot reject the null hypothesis that $\bar{\alpha}_l = 0$ precisely when $\frac{k}{2} \leq P^{TR}_l = F^{PL}(\bar{\alpha}_l) \leq 1 - \frac{k}{2}$. A $(1-k)$ percent confidence interval can then be found by inverting this test, asking for what values of ν does the adjusted effect $\bar{\alpha}_l - \nu$ appear free from treatment: when does $\frac{k}{2} \leq F^{PL}(\bar{\alpha}_l - \nu) \leq 1 - \frac{k}{2}$? The $(1-k)$ percent confidence interval for $\bar{\alpha}_l$ is the set of ν not rejected using the critical values $\frac{k}{2}$ and $1-\frac{k}{2}$, precisely the interval given in equation (1.14).[13]

I follow steps analogous to those above to conduct inference for $\bar{\alpha}$, the overall average effect of the fracking boom. This involves examining how large $\bar{\alpha}$ is relative to the distribution of the $N_{\overline{PL}}$ corresponding overall average placebo effects $\bar{\alpha}^{PL(c)}$. Specifically, the $(1-k)$ percent confidence interval of the overall average effect, $\bar{\alpha}$, is given by

$$\left(\bar{\alpha} - \bar{\alpha}^{PL(c)}_{(1-\frac{k}{2})}, \bar{\alpha} + \bar{\alpha}^{PL(c)}_{(\frac{k}{2})}\right). \tag{1.15}$$

[13]Figure 1.10 illustrates graphically step 3 of this procedure.

1.6 Results

In this section I test my theoretical model's predictions by first analyzing the effects of the fracking boom on educational outcomes, and then exploring potential mechanisms driving these effects.

1.6.1 College Educational Attainment - Repeated Cross Sections

Table 1.3 reports the average effects of fracking, $\bar{\alpha}$, on college educational outcomes from the ACS and IPEDS separately for men in panel A and women in panel B. In fracking boom counties, the proportion of men aged 18 to 26 enrolled in college decreases by 4.7 percentage points relative to their synthetic control over a ten year period following the start of a boom, a decrease of about 12.5 percent compared to the mean proportion enrolled of 37.6 percent. Women aged 18 to 26 in boom counties see a similar but slightly smaller reduction in the proportion enrolled in college. As a result of the fracking boom, the proportion of woman enrolled in college decreases by about 3.9 percentage points, a decrease of about 8.7 percent relative to the mean proportion enrolled of 44.6 percent.

Although the estimates of the effect of the boom on the proportion graduated in a county are negative for both men and women, placebo analysis suggests that I cannot reject the null hypothesis that $\bar{\alpha} = 0$ for either group. Taken together, the evidence on enrollment and graduation is suggestive that the fracking boom likely affected individuals most on the college enrollment margin rather than the completion margin. Individuals near the threshold ability level, θ^*, are those whose educational attainment is most affected by the fracking boom. It is plausible that absent fracking, had they enrolled in college, these individuals are those most likely to drop out of college.

I capture how the effects of the fracking boom on these educational outcomes evolve over time in Figures 1.11 and 1.12. Panel (a) in both figures show the trends in the proportion enrolled and the proportion graduated in the boom counties and their synthetic control, while panel (b) illustrates the gap between the outcome variable in the boom counties and that of their synthetic control in each period. In other words, panel (b) shows the period-specific effects, $\bar{\alpha}_l$, of the fracking boom on the outcome variables of interest from five years before the start of the boom to ten years after. The average value of fracking production per capita tends to increase dramatically for about five years after the start of a boom, at which point it hits a peak and then proceeds to steadily decline (see Figures 1.8 and 1.9). The negative effect of the fracking boom on the proportion of males enrolled follows a strikingly similar pattern, increasing in magnitude and reaching a low between three and five years following the start of a boom. During the peak producing years of a boom, the proportion of males enrolled is about 8 percentage points lower than the proportion enrolled in the synthetic control. A similar but less pronounced pattern can be seen for women. Another insight from the dynamic effects captured in these figures is that the large decline in male college enrollment three to five years after the start of the boom is followed by a decline of graduation rates six to nine years after the start of the boom. The overall average effects reported previously mask these lagged effects on male graduation rates.

Using only the ACS data, it would be unclear whether this reduction in college enrollment was driven by lower enrollment in four-year institutions, two-year institutions, or some combination of both. Identifying which individuals are most affected by fracking is important, and the IPEDS data are useful in this regard. Column three of Table 1.3 reports the effect of the fracking boom on the enrollment rate at four-year institutions. The fracking boom resulted in a 1.3 percentage point decrease in the male enrollment rate at four-year institutions, about an 11.6 percent reduction relative to the mean male enrollment rate of 11.2 percent. The estimated effect for the female enrollment rate at four-year institutions is larger in magnitude, a decrease of about 2 percentage points (about 15 percent relative to the mean female enrollment rate of 13.5 percent). The distribution of placebo estimates for the female enrollment rate has a relatively larger variance however, so despite the larger estimated effect, I cannot reject the null hypothesis that $\bar{\alpha} = 0$. I find no effect of fracking on graduation rates at four-year institutions for either men or women. In contrast to the negative enrollment effects at four-year institutions, the point estimates of the fracking boom on both enrollment and graduation rates at two-year institutions are positive for men and women, though the effects are smaller in magnitude and not statistically significant for men.

Figures 1.13, 1.14, 1.15, and 1.16 show the trends in male and female enrollment and graduation rates by level of institution, as well as the lead and lag specific effects of the fracking boom, $\bar{\alpha}_l$, on these outcomes. Enrollment rates at four-year institutions follow a similar pattern to those measured using the ACS for both men and women. Specifically, enrollment rates in boom counties decreased for several years relative to their synthetic control, this pattern reversing with the slow in fracking production. Although less pronounced, the enrollment and graduation rates at two-year institutions appear to move in tandem with fracking production during a boom as well.

Taken together, the evidence suggests that county-specific fracking booms acted to decrease college enrollments rates for both men and women, with the effects being relatively larger in magnitude for men. These negative effects on enrollment are driven primarily by decreases in enrollment rates at four-year institutions. Indeed, the evidence is suggestive that individuals, especially women, may have substituted away from four-year institutions and attended two-year institutions in these boom counties.

My findings complement those from Charles et al. (2018) on the housing boom and bust. Similar to Charles et al. (2018), I find that within areas experiencing a boom, college enrollment declines only temporarily. They find, however, that the effects of the housing boom on college enrollment are concentrated at two-year institutions. Apparently the source, locations, and type of industry-specific economic booms matter for their effect on educational attainment. The human capital required for jobs in the oil and gas industry can be highly specialized, and although many jobs related to construction, drilling, and extraction of oil and gas do not require a formal degree, a certificate or two-year degree can increase the likelihood of employment as well as the compensation associated with employment.

1.6.2 College Educational Attainment - Affected Cohorts

The transitory effect of fracking on enrollment rates within a county is not indicative of whether those that substituted away from college remained out of college or eventually attended. To analyze whether fracking had enduring effects on educational attainment, I redefine my sample to include only individuals aged 16 to 19 at the start of a boom, and then follow these cohorts as they age over the boom cycle. I choose these cohorts because they are of prime college going ages during the rapid increase in production phase of a boom.

The results in Table 1.4 show that on average over the ten year period following the start of a boom, men and women of these cohorts are less likely to enroll in college in boom counties relative to synthetic control counties. Figures 1.17 and 1.18 capture the dynamic effects of the fracking boom on enrollment and educational attainment of men and women from these cohorts. As these individuals age, they are less likely to enroll in college regardless of living in a boom county or not. On a given year during the peak years of a boom however, men and women in boom counties are significantly less likely to be enrolled than their counterparts in the synthetic control counties. Men and women aged 16 to 19 at the start of the boom are less likely to be enrolled throughout the duration of the boom, and no more likely to be enrolled after the boom. Following a drop in enrollment, one might expect to see fewer years of educational attainment as these cohorts age. Although the effects of the fracking boom on educational attainment are imprecisely estimated, I find evidence that these individuals end up with fewer years of completed schooling than they otherwise would have absent the fracking boom. Taken together, these results suggest that the effects of fracking on these affected cohorts appear to be permanent, despite the transitory nature of the booms.

1.6.3 Mechanisms

1.6.3.1 Shocks to Local Labor Demand

I find strong evidence consistent with my theoretical model's prediction that increasing the opportunity cost of attending college and decreasing the relative returns to college will reduce educational attainment. The opportunity cost of going to college refers to the potential foregone earnings of a worker without a college degree, which I measure using the average annual earnings of non-college-educated individuals. As a measure of the expected future college premium, I use the ratio of average annual earnings of college to non-college-educated workers. In this section I provide evidence that both the demand for college-educated and non-college-educated labor increased in fracking boom counties, with the shock to non-college-educated labor being relatively larger than to college-educated labor. This implies that there was not only an increase in the opportunity cost of going to college, but that the expected future college premium decreased in these areas. Both of these findings are consistent with the findings related to educational attainment in the previous section.

Table 1.5 reports separately for both men and women, the average effects of fracking, $\bar{\alpha}$, on average annual earnings and the jobs-to-population ratio. The fracking boom increased male earnings by a substantial 15.5 percent, and female earnings by about 6.5 percent. The effect is larger for non-college-educated men and women (16.2 and 9.4 percent) than for college-educated men and women (11.3 and 5.3 percent). The average earnings for non-college-educated workers in the sample is \$33,745. Therefore, average earnings of non-college-educated workers increased by an average of nearly \$5,500 per year over the ten years following the start of a boom. The fracking boom also increased the jobs-to-population ratio for men and women, with the effect again being larger in magnitude for men (13 percent) than for women (3.1 percent). These effects on employment were also especially large for non-college-educated workers relative to college-educated workers, regardless of gender.

Figures 1.19, 1.20, and 1.21 show the trends in the natural log of average annual earnings and the natural log of the jobs-to-population ratio of all, college-educated, and non-college-educated male workers in fracking boom counties and their synthetic controls, as well as the lead and lag specific effects of fracking, $\bar{\alpha}_l$, on these two outcomes. For all three groups of men, the effect on earnings increased steadily for the first five years following the boom, then remained relatively constant at that level over the remaining five years. The figures indicate that there was a boom in male employment that moved congruent with the boom in fracking production; increasing substantially for about five years, then decreasing over the following couple of years. Here again we see that in each period during the treatment period, the lag specific effects on earnings and employment are relatively larger for non-college-educated males than college-educated males. Though less pronounced, similar dynamic effects can be seen for women in Figures 1.22, 1.23, and 1.24. One exception in Figure 1.23 is the null effect on the jobs-to-population ratio of college-educated women. Importantly, my measures of earnings and employment of college-educated individuals refer to those with at least a bachelor's degree. Thus, this null result is not necessarily surprising given the observed increase in female two-year college enrollment rates in these counties.

The effects of the fracking boom on earnings and employment were consistently larger for non-college-educated workers than college-educated workers. In Table 1.6 and Figures 1.25 and 1.26, I look directly at the overall average effect of the fracking boom, $\bar{\alpha}$, on the college premium and college-to-non-college-educated employment ratio. I find that due to the fracking boom, the male college premium decreased by 6.7 percent and the male employment ratio decreased by 10.7 percent. The female college premium and college-to-non-college employment ratio also decreased, though the effects are again less pronounced. In addition to the considerable increase in the opportunity cost of going to college, the reduction in the expected future returns of a college degree disincentivized investment in a college education.

My theoretical model provides guidance on how to identify empirically the extent to which the fracking boom affected educational outcomes through these labor market channels specifically. This involved identifying two groups that experienced the same effects of fracking on the indirect costs of going to college, with only one group experiencing the opportunity cost and college premium effects of fracking. By differencing the reduced from effects across these two groups, the effects of fracking working through these two labor market channels would be identified (see Equation (1.5)). Although women did experience the labor market effects of fracking, these effects were consistently smaller in magnitude than the labor market effects for men. This fact, together with smaller-in-magnitude effects of fracking on educational outcomes of women compared to men provide convincing evidence that an increase in the opportunity cost of going to college, as well as a reduction in the expected future college premium are important channels through which fracking affected college educational outcomes.

1.6.3.2 Migration

One important alternative is that my estimated effects are simply picking up a change in the composition of the population due to in-migration. By restricting the ACS sample to individuals aged 18 to 26, I remove the possibility that my estimated effects are being driven by individuals not of the common college going ages. Suppose however, that there is a group of individuals aged 18 to 26 who moved into fracking boom counties as a result of the boom. Suppose further that these individuals would not have attended college even if there was no boom. If these individuals migrated from potential control counties, then all else equal the proportion of individuals enrolled in the boom county would decrease and the proportion of individuals enrolled in the potential control county would increase. To identify the extent to which changes in the composition of the population influence my estimated effects, I restrict the ACS sample further to only include individuals who reported not having moved since prior to any fracking boom. By restricting the sample in this way, I identify the effects of fracking on the college educational attainment decisions of long-term residents of the boom counties, compared to those of the long-term residents of the potential control counties.

Figures 1.27 and 1.28 show trends in the proportion of long-term resident men and women aged 18 to 26 enrolled and graduated in boom counties and their synthetic control counties, as well as the lead and lag specific effects of fracking, $\bar{\alpha}_l$, on these outcomes. In fracking boom counties, the proportion of long-term resident men and women enrolled in college decreased by 4.7 and 2.4 percentage points, respectively, relative to their synthetic controls over a ten year period following the start of the boom (see Table 1.7). These overall average effects on college enrollment, as well as the dynamic effects are very similar to those from the unrestricted sample of all individuals aged

18 to 26. The average and dynamic effects of fracking on graduation rates are also very similar between the restricted sample of long-term residents and the unrestricted sample. Table 1.4, Figure 1.29, and Figure 1.30 also show that the average and dynamic effects of fracking on the educational outcomes of long-term residents aged 16 to 19 at the start of the boom are very similar to those from the unrestricted sample of all individuals aged 16 to 19 at the start of the boom. This evidence supports the conclusion that previous results are not simply a reflection of changes in the composition of the population as the booms in fracking unfolded, but rather a consequence of fracking booms influence on individuals' decisions to attend college.

1.7 Conclusion

To identify the ways that the fracking boom has affected educational attainment, I use a comprehensive data set of oil and natural gas production to identify which counties experienced a boom and in what year the boom began in each county. I then use the synthetic control method to estimate the average and dynamic effects of these county-specific fracking booms on college investment decisions. I find that a boom in fracking production within a county causes a reduction in college enrollment, with the effect being concentrated among individuals at four-year institutions. Although the decline in college enrollment during a boom was generally reversed as fracking production slowed in a county, college attainment remained persistently low for cohorts in their early 20s during the rise and peak of a boom.

My theoretical model illustrates mechanisms through which the fracking boom would affect educational outcomes. I find evidence in support of that model's predictions. A boom in fracking production increases the earnings and employment of both non-college and college-educated workers, with relatively larger effects for non-college-educated workers. The fracking booms thus not only increase the opportunity cost of additional years of schooling, but also decrease the expected relative returns to additional years of schooling.

The literature on resource and other localized economic booms suggest a variety of plausible causal links to educational attainment. Although I focus primarily on the college premium, two other relevant routes are migration and changes in parental and government resources. The estimated effects of the fracking boom on college educational attainment in this paper are not being driven by changes in the composition of the population. Instead, I find that fracking decreased college investment of long-term residents in boom counties by increasing the opportunity cost of, and decreasing the relative returns to, schooling. If fracking did result in more financial resources for education, then that does not dominate the effects of the increased opportunity cost of education.

Table 1.1: Boom County Event Years

County	State	Event Year	County	State	Event Year
Conecuh	AL	2008	Seminole	OK	2003
Cleburne	AR	2009	Stephens	OK	2010
Conway	AR	2006	Washita	OK	2005
Faulkner	AR	2006	Bradford	PA	2009
Van Buren	AR	2006	Clinton	PA	2007
White	AR	2005	Tioga	PA	2009
Garfield	CO	2003	Harding	SD	2003
Rio Blanco	CO	2005	Andrews	TX	2009
Bossier	LA	2006	Borden	TX	2003
Caddo	LA	2006	Calhoun	TX	2002
Cameron	LA	2003	Colorado	TX	2009
De Soto	LA	2006	Denton	TX	2003
Evangeline	LA	2007	Ector	TX	2006
Lafourche	LA	2003	Freestone	TX	2000
Plaquemines	LA	2002	Frio	TX	2009
Red River	LA	2006	Gaines	TX	2009
St Mary	LA	2003	Grimes	TX	2003
Vermilion	LA	2001	Hardeman	TX	2000
Webster	LA	2001	Harrison	TX	2006
Jasper	MS	2003	Hemphill	TX	2006
Lincoln	MS	2003	Jack	TX	2003
Wayne	MS	2006	Jasper	TX	2006
Blaine	MT	2003	Jefferson	TX	2005
Dawson	MT	2001	Johnson	TX	2003
Fallon	MT	2003	Leon	TX	2005
Richland	MT	2003	Lipscomb	TX	2003
Roosevelt	MT	2008	Live Oak	TX	2009
Sheridan	MT	2003	Montague	TX	2009
Wibaux	MT	2003	Nacogdoches	TX	2007
Eddy	NM	2009	Ochiltree	TX	2009
Rio Arriba	NM	2006	Orange	TX	2000
Billings	ND	2009	Panola	TX	2009
Bottineau	ND	2009	Parker	TX	2003
Bowman	ND	2003	Pecos	TX	2012
Burke	ND	2009	Roberts	TX	2009
Divide	ND	2008	Robertson	TX	2001
Dunn	ND	2009	Rusk	TX	2009
Golden Valley	ND	2009	San Augustine	TX	2006
Grand Forks	ND	2009	Shelby	TX	2006
McLean	ND	2007	Terry	TX	2005
Mountrail	ND	2007	Tyler	TX	2003
Renville	ND	2003	Ward	TX	2009
Stark	ND	2008	Webb	TX	2009
Williams	ND	2009	Willacy	TX	2000
Blaine	OK	2012	Wise	TX	2003
Canadian	OK	2009	Carbon	UT	2007
Carter	OK	2009	Duchesne	UT	2009
Coal	OK	2005	Uintah	UT	2009
Dewey	OK	2008	Upshur	WV	2006
Ellis	OK	2007	Carbon	WY	2003
Hughes	OK	2003	Hot Springs	WY	2003
Johnston	OK	2009	Park	WY	2003
Marshall	OK	2009	Sublette	WY	2003
Pittsburg	OK	2006	Sweetwater	WY	2009

Notes: The event year for each county is defined as the year in the sample in which fracking increased the most over the following five year period.

Table 1.2: Descriptive Statistics

	Fracking Boom Counties			Non-Fracking Counties		
	Mean	S.D.	Median	Mean	S.D.	Median
ACS						
Prop. Enrolled (%)	33.8	12.6	33.7	37	16.2	18
Prop. Graduated (%)	7.5	4.2	7.2	8.7	5.6	8.1
Observations:		1,404			16,412	
IPEDS						
Two-Year Colleges						
Enrollment Rate (%)	15.6	15.2	10	9.6	19.7	5.2
Graduation Rate (%)	4.5	4.5	2.9	3.2	6.4	1.2
Observations:		475			6,948	
Four-Year Colleges						
Enrollment Rate (%)	13.6	8.6	14.4	13.1	10.6	10.8
Graduation Rate (%)	5.3	4.3	4	6.1	4.8	4.8
Observations:		280			6,717	
QWI						
All Workers						
Ave. Earnings	34,077	6,557	33,158	32,585	6,887	31,253
Jobs/Population	.333	.120	.308	.340	.127	.327
Non-college Workers						
Ave. Earnings	30,207	6,489	28,904	27,375	4,350	26,890
Jobs/Population	.135	.052	.124	.130	.043	.127
College Workers						
Ave. Earnings	50,278	10,058	49,869	50,496	11,117	48,555
Jobs/Population	.056	.020	.053	.066	.038	.056
Observations:		1,916			22,520	
Population Characteristics						
Prop. Male (%)	50.3	2	49.7	49.8	2	49.5
Prop. White Male (%)	85.7	13.2	90.9	85.9	16.3	93
Prop. White Female (%)	85.7	14	91.8	85.8	17.3	93.9
Prop. Male Aged 20 to 34 (%)	18.7	3.4	18.4	18.6	4.2	17.9
Prop. Female Aged 20 to 34 (%)	17	3.1	17.1	17.2	3.5	16.8
Observations:		1,916			22,520	

Notes: The unit of observation is county-year. Average annual earnings are in 2010 dollars. Data sources: 2005-2017 American Community Survey (ACS), 2000-2016 Integrated Postsecondary Education Data System (IPEDS), 2000-2016 Quarterly Workforce Indicators (QWI), and the 2000-2017 U.S. Census Population Estimates.

Table 1.3: Overall Average Effects of Fracking on Educational Outcomes

	ACS		IPEDS			
	Proportion Enrolled (1)	Proportion Graduated (2)	Four-Year Enrollment Rate (3)	Four-Year Graduation Rate (4)	Two-Year Enrollment Rate (5)	Two-Year Graduation Rate (6)
***Panel A.* Males**						
Average Effect $\bar{\alpha}$ (p.p.)	-4.69	-0.71	-1.30	-0.02	0.50	0.52
95% Confidence Interval	[-7.01,-1.69]	[-1.25,0.43]	[-3.03,0.01]	[-0.62,0.54]	[-0.57,1.13]	[-0.55,1.15]
Baseline Average (%)	37.60	7.72	11.23	5.27	9.71	3.05
***Panel B.* Females**						
Average Effect $\bar{\alpha}$ (p.p.)	-3.87	-0.13	-2.02	0.15	1.15	0.96
95% Confidence Interval	[-6.43,-1.16]	[-0.89,1.34]	[-5.07,1.29]	[-0.88,0.83]	[0.52,1.70]	[0.33,1.51]
Baseline Average (%)	44.61	12.46	13.50	7.34	10.81	3.36

Notes: This table reports overall average effects of fracking on educational outcomes ($\bar{\alpha}$), measured in percentage points (p.p.), from equation (1.13). The 95 percent confidence intervals are estimated using equation (1.15) following the steps outlined in section 1.5.3. Also reported are the baseline average values, measured in percentages, of the various educational outcome variables. Data sources: 2005-2017 American Community Survey (ACS) and 2000-2016 Integrated Postsecondary Education Data System (IPEDS).

Table 1.4: Overall Average Effects of Fracking on Educational Outcomes (Individuals Aged 16 to 19 at the Start of the Boom)

	Prop. Enrolled$_{cy}$		*Educational Attainment$_{cy}$* **(***Years Completed***)**	
	Males	Females	Males	Females
	(1)	(2)	(3)	(4)
***Panel A.* All Individuals**				
Average Effect $\bar{\alpha}$	-3.92	-3.67	-0.11	-0.10
95% Confidence Interval	[-5.51,-1.70]	[-5.42,-1.78]	[-0.31,0.29]	[-0.30,0.28]
Baseline Average	55.08	59.96	11.14	11.44
***Panel B.* Long-Term Residents**				
Average Effect $\bar{\alpha}$	-3.63	-2.66	-0.16	-0.11
95% Confidence Interval	[-5.20,-1.59]	[-4.34,-0.71]	[-0.38,0.23]	[-0.32,0.24]
Baseline Average	55.83	61.11	11.07	11.40

Notes: This table reports overall average effects of fracking on college enrollment and educational attainment $\bar{\alpha}$, measured in percentage points and years of completed education, from equation (1.13). Long-term residents are those who have been in the same residence since prior to any boom in fracking production. The 95 percent confidence intervals are estimated using equation (1.15) following the steps outlined in section 1.5.3. Also reported are the baseline average values of college enrollment and educational attainment, measured in percentages and years of completed education. Data source: 2005-2017 American Community Survey.

Table 1.5: Overall Average Effects of Fracking on Labor Market Outcomes

	Average Annual Earnings$_{cy}$			*Jobs / Population*$_{cy}$		
	All	College Educated	Non-College Educated	All	College Educated	Non-College Educated
	(1)	(2)	(3)	(4)	(5)	(6)
***Panel A.* Males**						
Average Effect $\bar{\alpha}$ (%Δ)	15.47	11.32	16.18	12.99	6.46	15.54
95% Confidence Interval	[14.04,16.82]	[9.27,12.95]	[14.93,17.50]	[11.37,16.37]	[5.29,10.03]	[13.64,18.82]
Baseline Average	39,639	62,349	33,745	0.34	0.062	0.14
***Panel B.* Females**						
Average Effect $\bar{\alpha}$ (%Δ)	6.47	5.32	9.40	3.07	-1.26	5.51
95% Confidence Interval	[5.23,7.39]	[3.74,6.08]	[7.93,10.25]	[2.11,5.71]	[-1.92,1.93]	[4.37,8.32]
Baseline Average	25,353	38,227	21,121	0.33	0.033	0.12

Notes: This table reports overall average effects of fracking, $\bar{\alpha}$ from equation (1.13), on the average annual earnings (measured in 2010 dollars) and jobs-to-population ratios of all, college educated, and non-college educated men and women. The 95 percent confidence intervals are estimated using equation (1.15) following the steps outlined in section 1.5.3. Also reported are the baseline average values of the average annual earnings and the jobs-to-population ratios for each group. Data source: 2000-2016 Quarterly Workforce Indicators.

Table 1.6: Overall Average Effects of Fracking on the College Premium and the College-to-Non-College Educated Employment Ratio

	College Premium$_{cy}$		*College / Non College Employment$_{cy}$*	
	Males (1)	Females (2)	Males (3)	Females (4)
Average Effect $\tilde{\alpha}$ (%Δ)	-6.70	-1.94	-10.74	-7.39
95% Confidence Interval	[-6.31,-9.94]	[-1.88,-4.24]	[-11.53,-8.53]	[-7.40,-4.83]
Baseline Average	1.85	1.82	0.44	0.55

Notes: This table reports overall average effects of fracking, $\tilde{\alpha}$ from equation (1.13), on the college premium (measured in 2010 dollars) and the employment ratio of college-to-non-college educated men and women. The 95 percent confidence intervals are estimated using equation (1.15) following the steps outlined in section 1.5.3. Also reported are the baseline average values of the college premiums and employment ratios. Data source: 2000-2016 Quarterly Workforce Indicators.

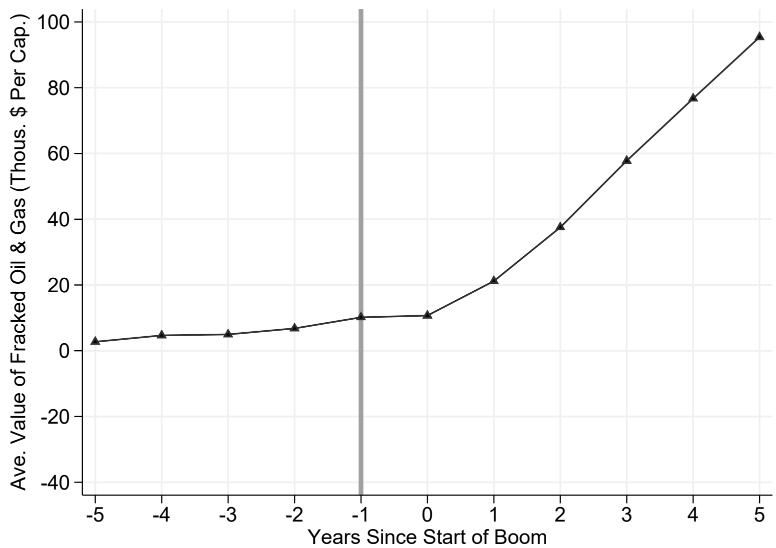
Table 1.7: Overall Average Effects of Fracking on Long-Term Resident Educational Outcomes

	Prop. Enrolled$_{cy}$		*Prop. Graduated*$_{cy}$	
	Males	Females	Males	Females
	(1)	(2)	(3)	(4)
Average Effect $\bar{\alpha}$ (p.p.)	-4.9	-2.79	-0.93	-0.06
95% Confidence Interval	[-7.22,-1.89]	[-5.35,-0.08]	[-1.47,0.21]	[-0.82,1.41]
Baseline Average	41.14	48.97	5.78	10.62

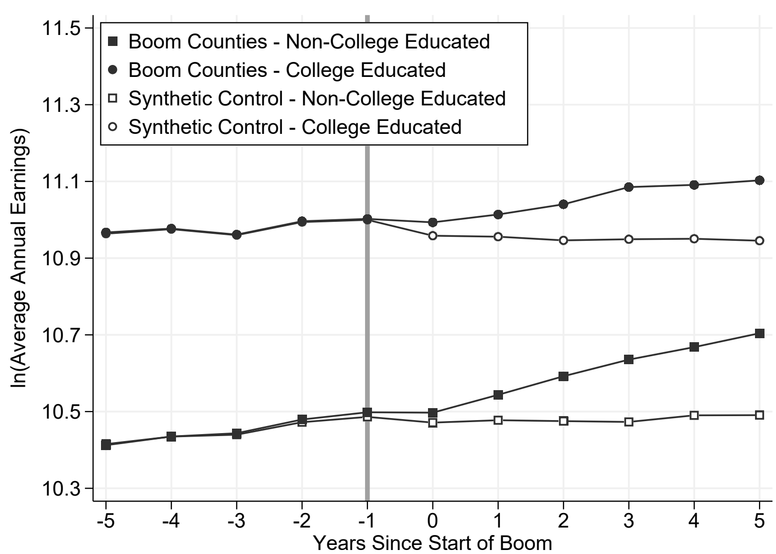
Notes: This table reports overall average effects of fracking on educational outcomes of long-term residents $\bar{\alpha}$, measured in percentage points (p.p.), from equation (1.13). Long-term residents are those who have been in the same residence since prior to any boom in fracking production. The 95 percent confidence intervals are estimated using equation (1.15) following the steps outlined in section 1.5.3. Also reported are the baseline average values, measured in percentages, of the various educational outcome variables. Data source: 2005-2017 American Community Survey.

Figure 1.1: Fracking Production and Earnings by Educational Attainment

(a) Average Value of Fracking Production Per Capita in Boom Counties

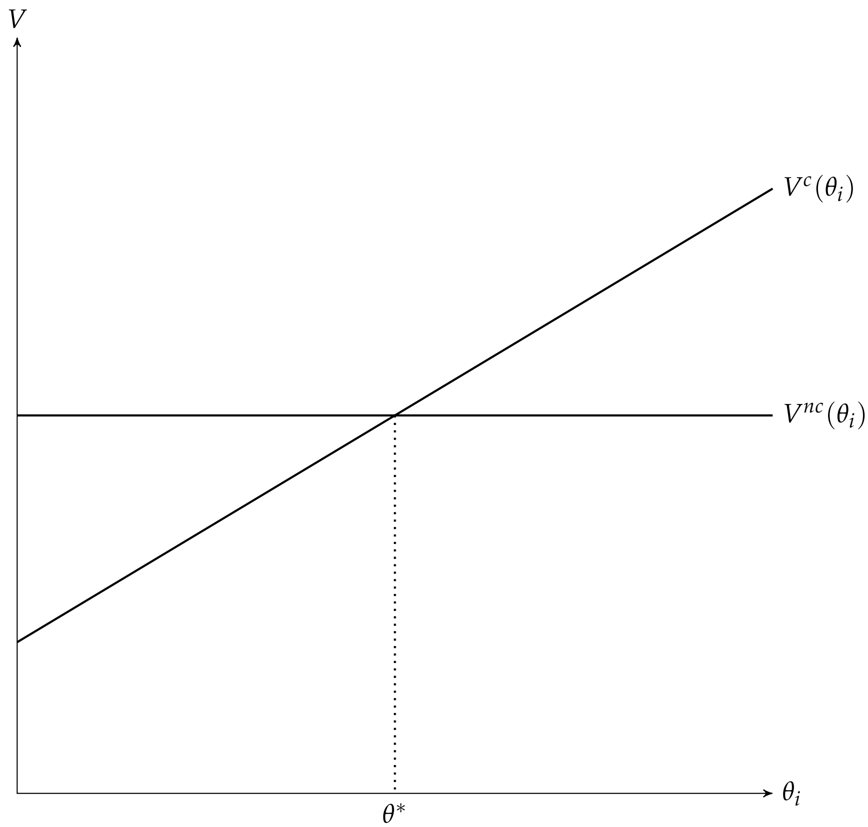


(b) Earnings by Educational Attainment in Boom Counties and Their Synthetic Controls



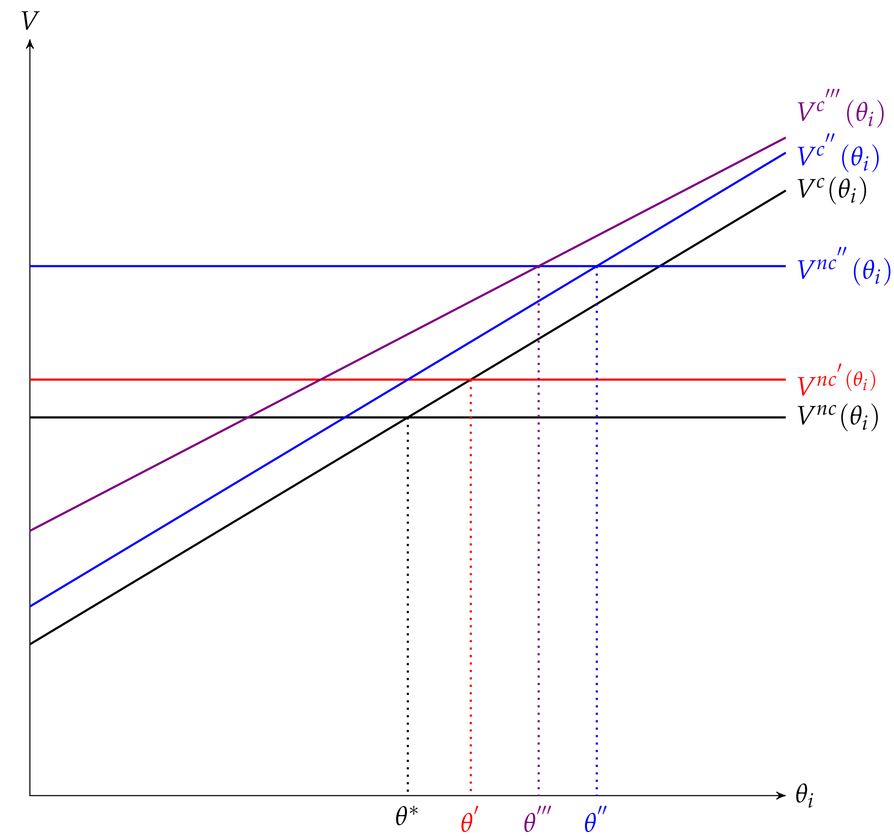
Notes: This figure contains the average value of fracked oil and gas production, measured in thousands of dollars per capita, in boom counties (panel (a)). Panel (b) shows the natural log of average annual earnings of non-college-educated and college-educated workers, in boom counties and their synthetic controls. The synthetic control counties are estimated according to the methodology outlined in Section 1.5. Data sources: DrillingInfo and the 2000-2016 Quarterly Workforce Indicators.

Figure 1.2: Graphical Representation of Model Equilibrium



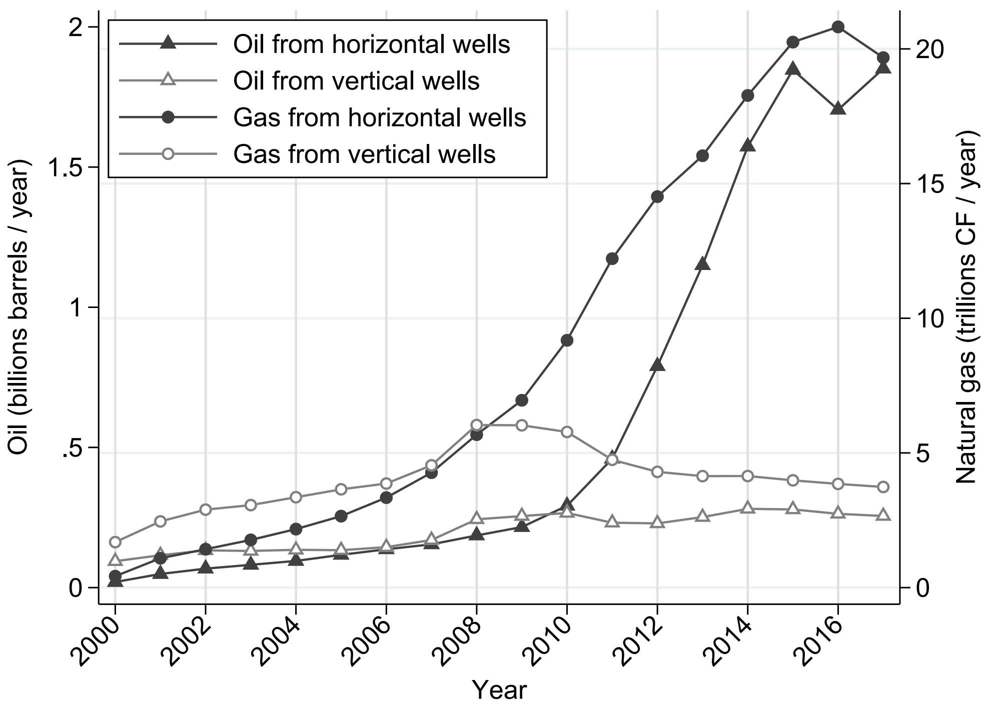
Notes: θ^* is the threshold ability level such that individuals with $\theta_i < \theta^*$ will not go to college, individuals with $\theta_i > \theta^*$ will go to college, and individuals with $\theta_i = \theta^*$ are indifferent between college or not.

Figure 1.3: Graphical Representation of Model Equilibrium with Fracking-Induced Shocks



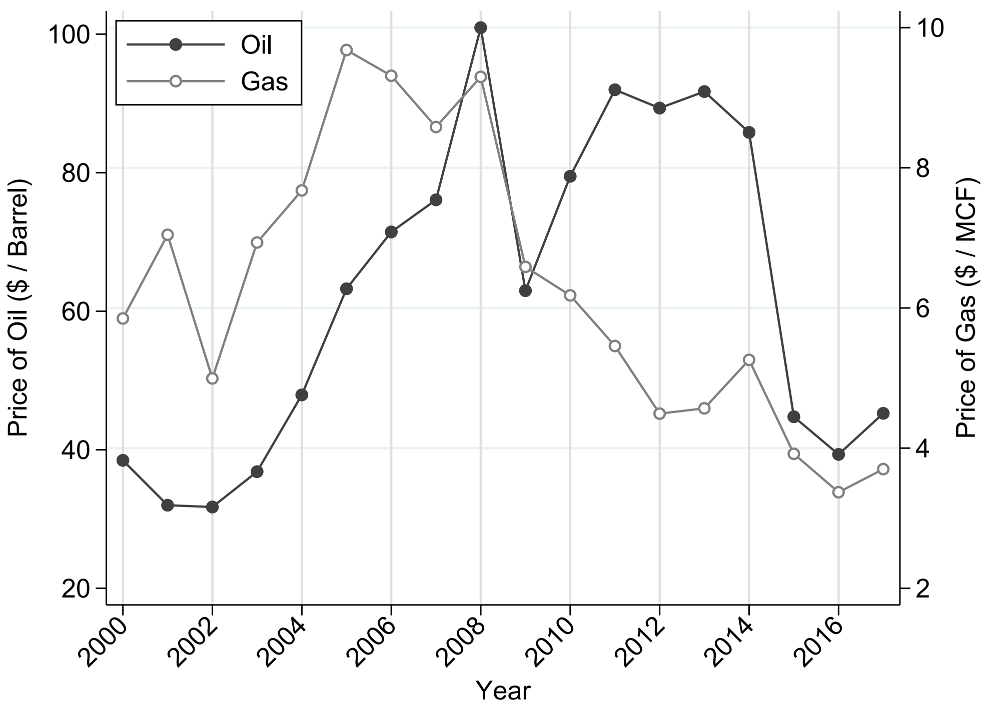
Notes: An increase in the opportunity cost of going to college would increase the threshold ability level to θ', resulting in fewer college attendees. A larger increase in the expected lifetime labor market income of non-graduates relative to graduates would lead to a decrease in the college income premium, further increasing the threshold ability level to θ'', resulting in even fewer college attendees. A relatively larger decrease in indirect costs for low ability individuals than high ability individuals would decrease the threshold ability level to θ'''.

Figure 1.4: U.S. Production by Drill Type



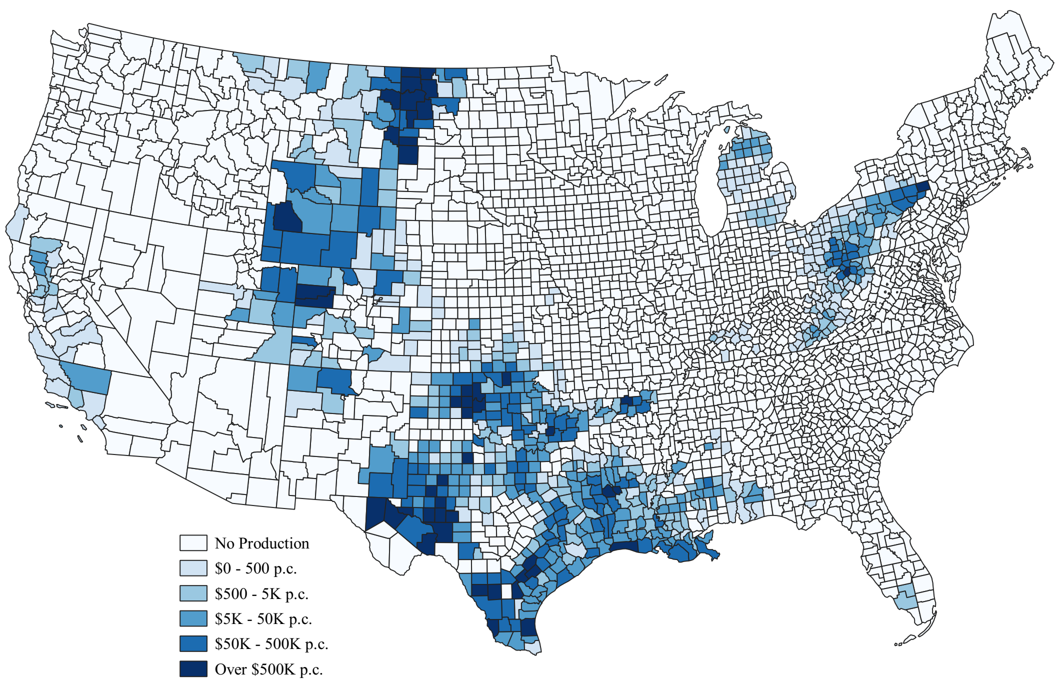
Notes: This figure contains yearly aggregates of oil and gas production with a drilling type of vertical or horizontal (including directional). These aggregates come from wells with first production date in the year 2000 or later. Data source: Drillinginfo.

Figure 1.5: Real Oil & Gas Prices



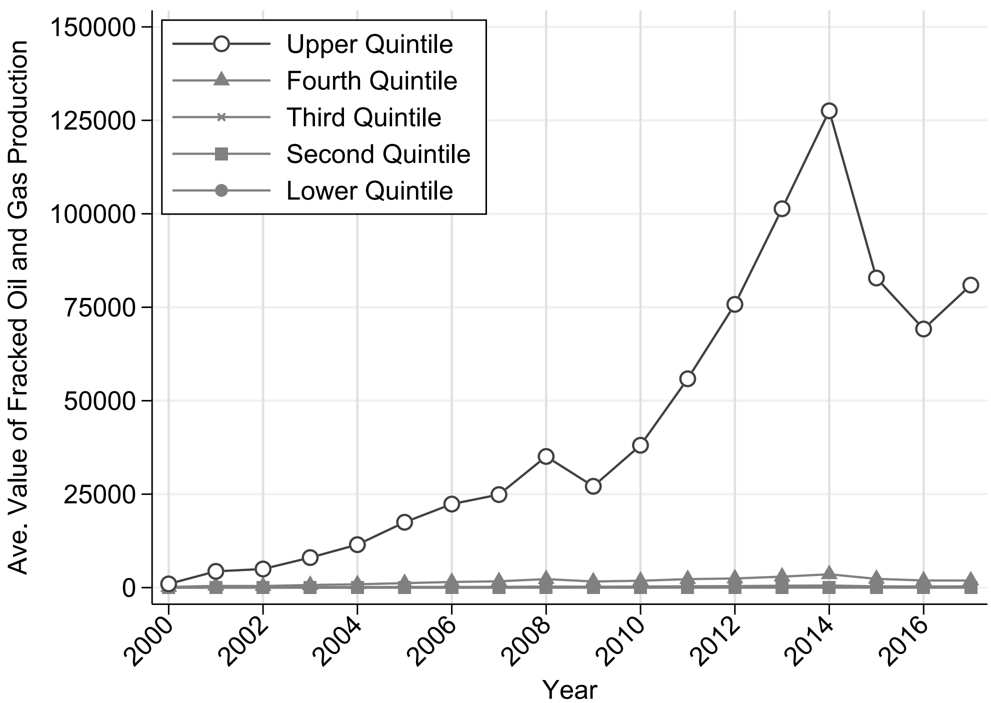
Notes: This figure contains average annual real prices of oil and gas production (in 2010 $). Data source: Energy Information Administration (EIA).

Figure 1.6: U.S. Fracking Production by County (2000 - 2017)



Notes: This figure contains the total value per capita of fracked oil and gas production by county from 2000 to 2017. Data source: Drillinginfo.

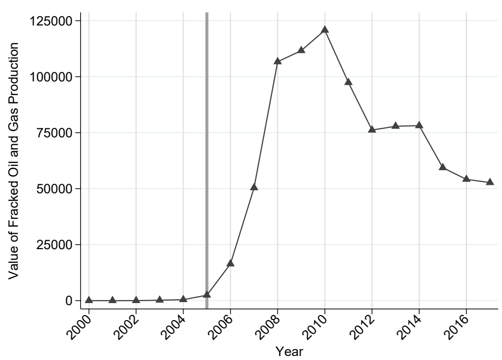
Figure 1.7: Annual Value of Fracked Oil and Gas Production by Quintile (2000 - 2017)



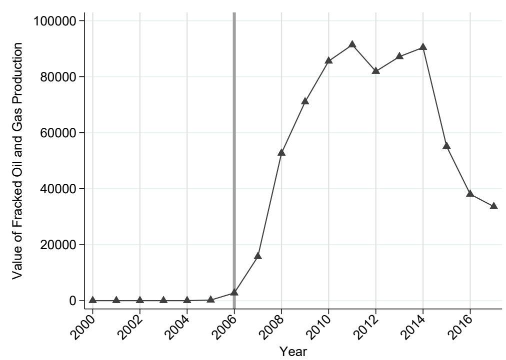
Notes: This figure contains the annual value of fracked oil and gas production per capita by quintile from 2000 to 2017. Data source: Drillinginfo.

Figure 1.8: Boom County Event Year Examples

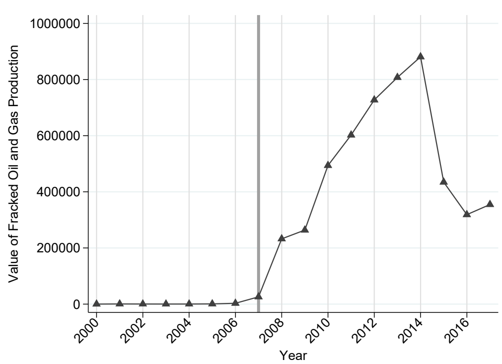
(a) Coal County, OK



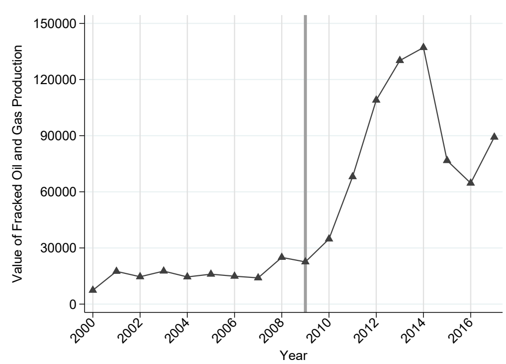
(b) Van Buren County, AR



(c) Mountrail County, ND

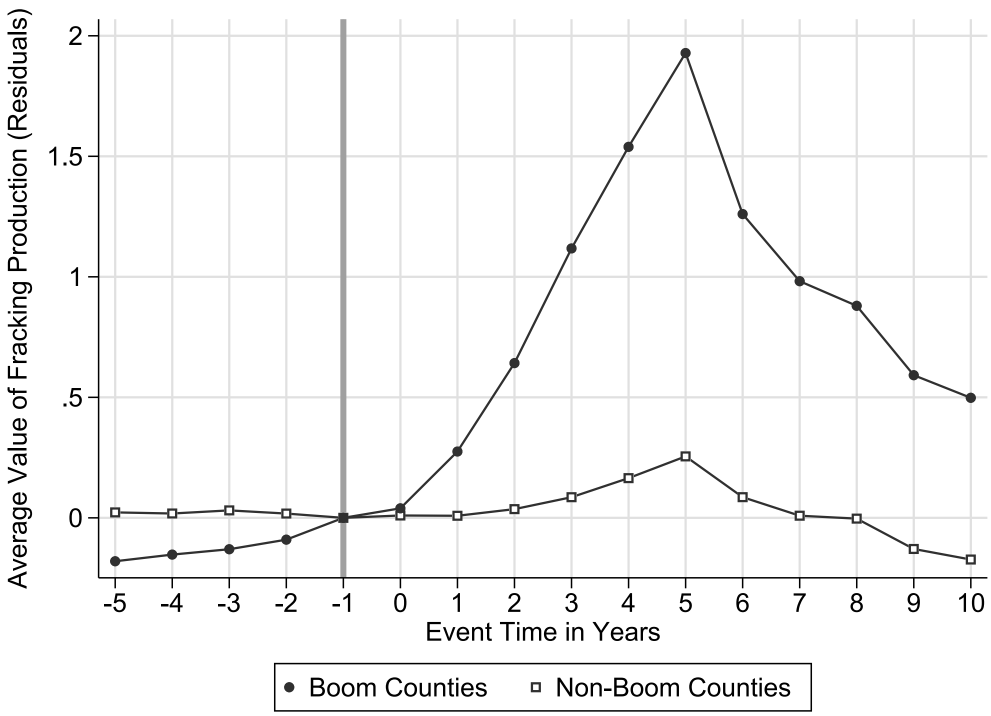


(d) Ward County, TX



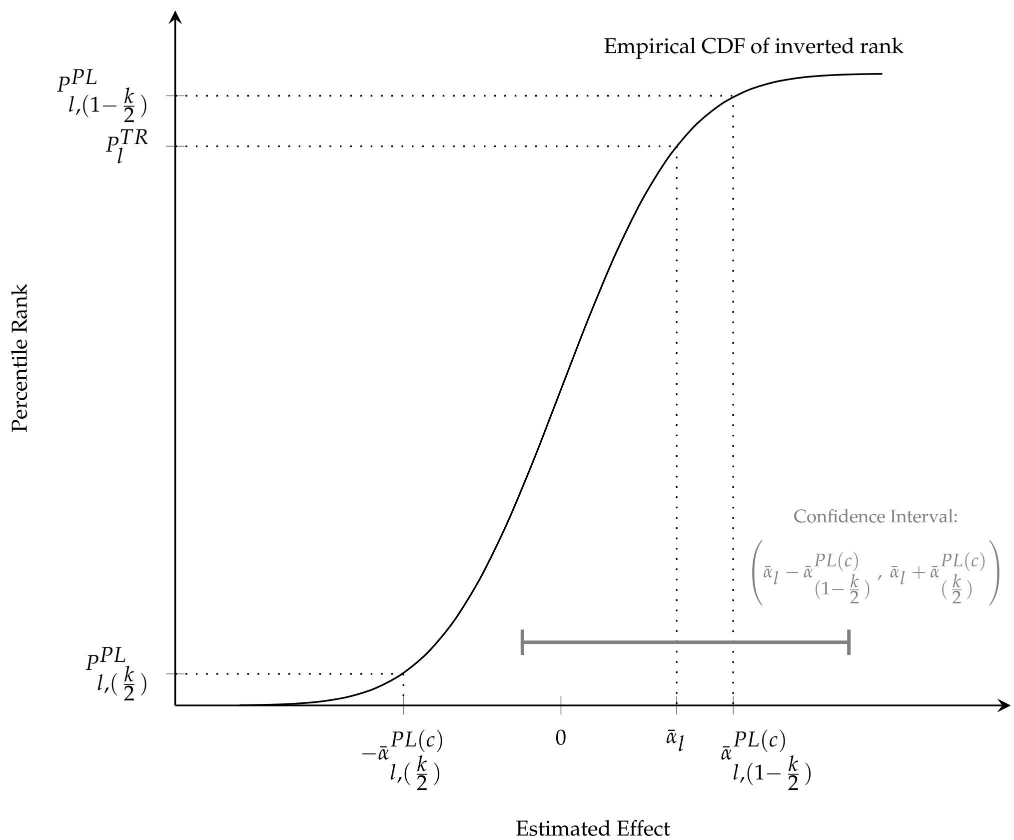
Notes: The vertical lines indicate the event year, defined as the year in the sample in which fracking increased the most over the following five year period. The event years for all boom counties can be found in Table 1.1.

Figure 1.9: Residual Fracking Production in Boom Counties and Non-Boom Counties



Notes: This figure contains the average residual value of fracked oil and gas production per capita from equation (1.6), in boom counties and non-boom counties. Residuals have been standardized and are shown relative to the year prior to the start of the boom. Data source: Drillinginfo.

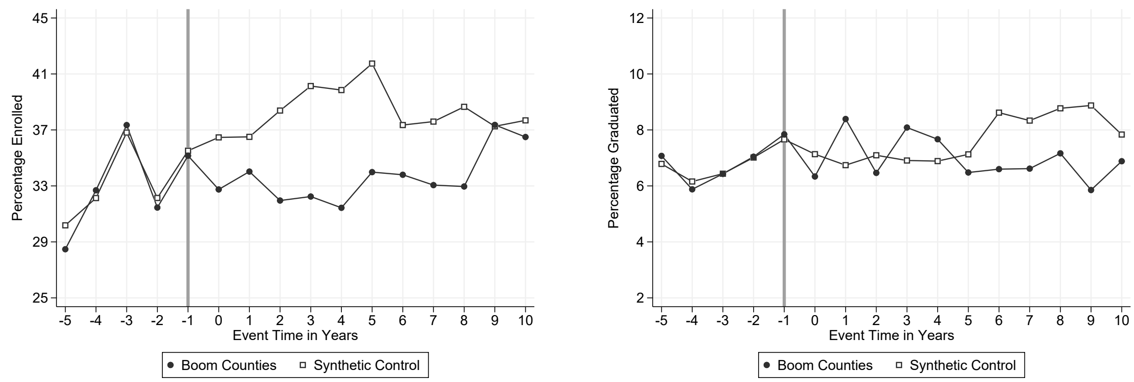
Figure 1.10: Formulation of Confidence Intervals by Inverting the Percentile Rank



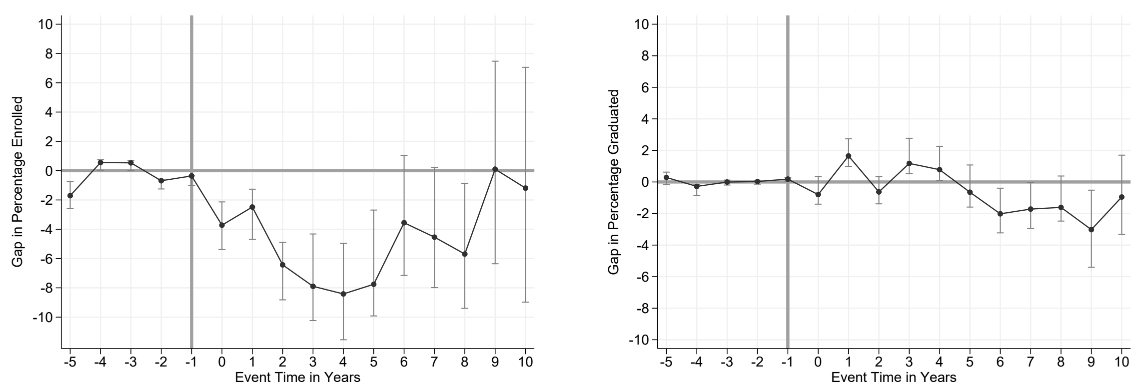
Notes: This figure illustrates how I calculate a $(1-k)$ percent confidence interval by inverting the percentile rank and determining for what values of ν the adjusted effect $(\bar{\alpha}_l - \nu)$ appears free from treatment. The $(1-k)$ percent confidence interval for $\bar{\alpha}_l$ is the set of ν not rejected using the critical values $\frac{k}{2}$ and $1-\frac{k}{2}$.

Figure 1.11: College Enrollment and Graduation Rates of Males Aged 18 to 26

(a) Trends



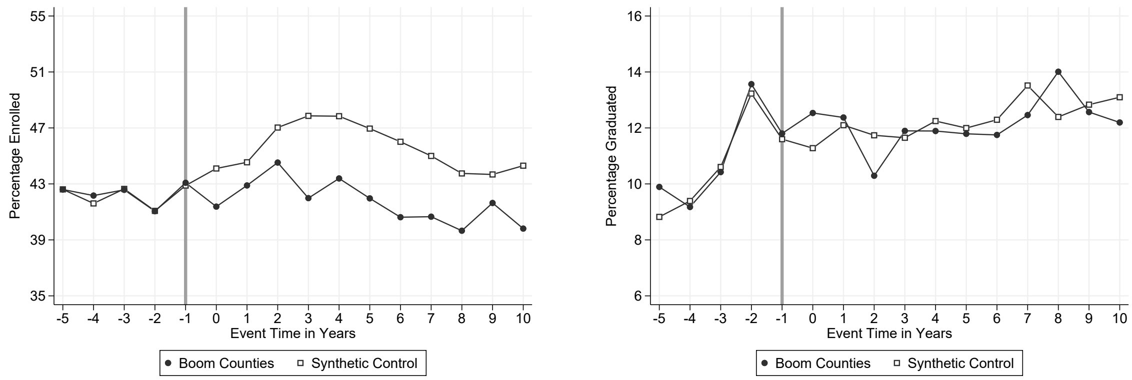
(b) Gap Between Boom Counties and their Synthetic Control



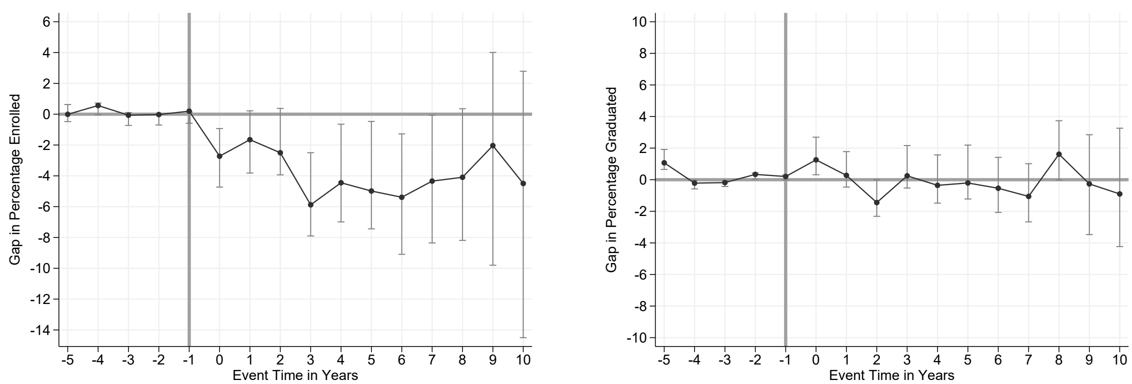
Notes: Panel (a) shows trends in the proportion of men enrolled in and graduated from college in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on the proportion of men enrolled in and graduated from college, $\bar{\alpha}_l$, measured in percentage points (p.p.), from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data source: 2005-2017 American Community Survey (ACS).

Figure 1.12: College Enrollment and Graduation Rates of Females Aged 18 to 26

(a) Trends



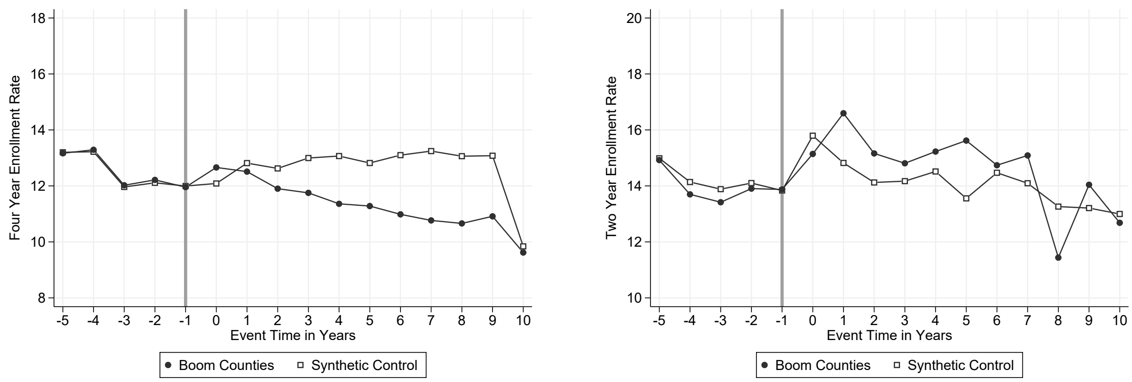
(b) Gap Between Boom Counties and their Synthetic Control



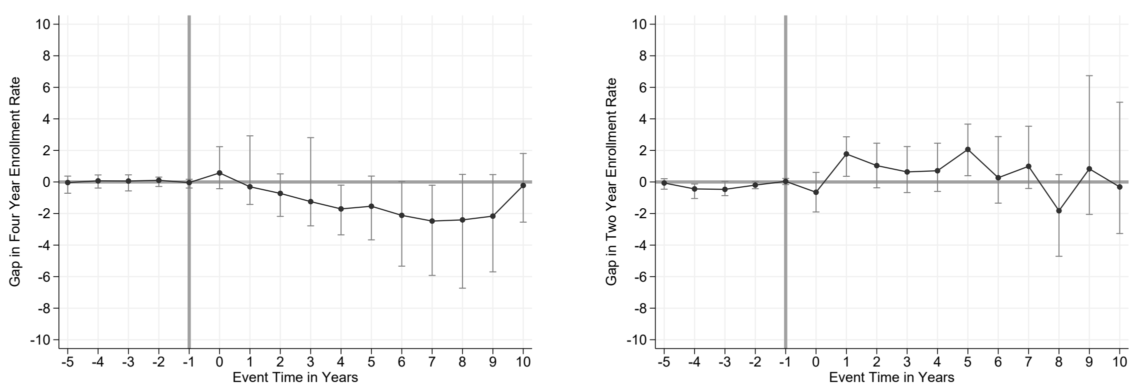
Notes: Panel (a) shows trends in the proportion of women enrolled in and graduated from college in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on the proportion of women enrolled in and graduated from college, $\bar{\alpha}_l$, measured in percentage points (p.p.), from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data source: 2005-2017 American Community Survey (ACS).

Figure 1.13: Male College Enrollment Rates by Level of Institution

(a) Trends



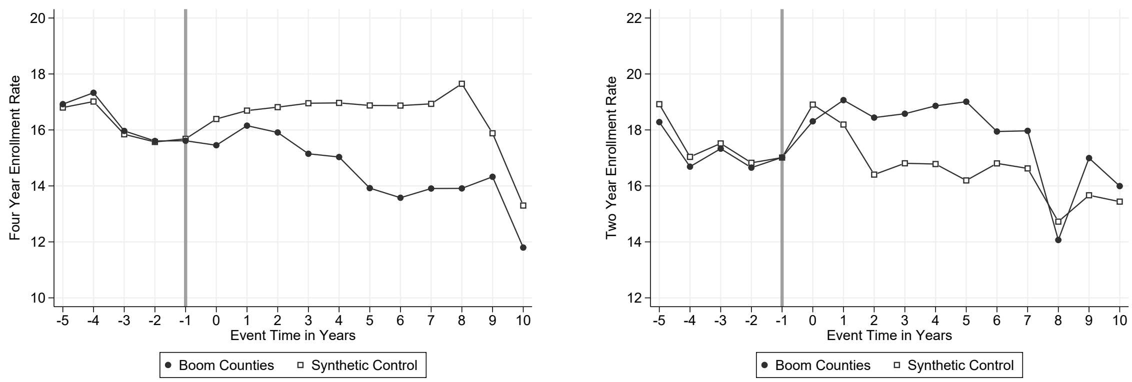
(b) Gap Between Boom Counties and their Synthetic Control



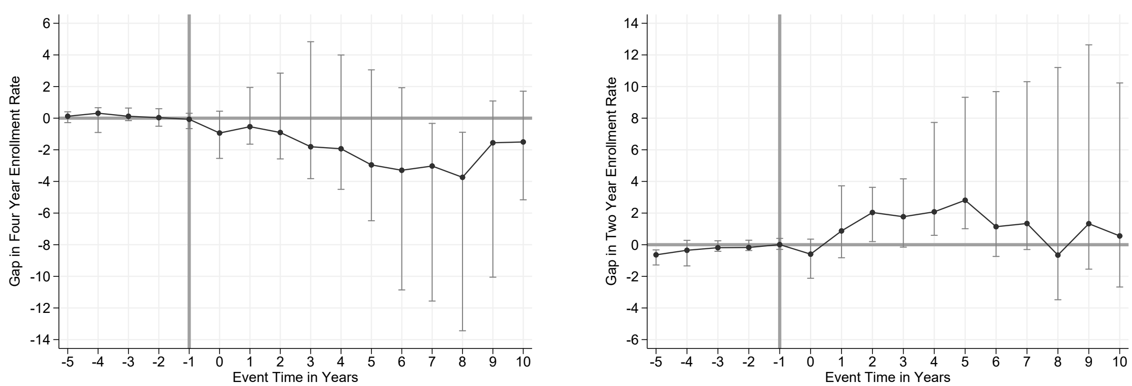
Notes: Panel (a) shows trends in male college enrollment rates by level of institution in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on male college enrollment rates, $\bar{\alpha}_l$, measured in percentage points, from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data source: 2000-2016 Integrated Postsecondary Education Data System (IPEDS).

Figure 1.14: Female College Enrollment Rates by Level of Institution

(a) Trends



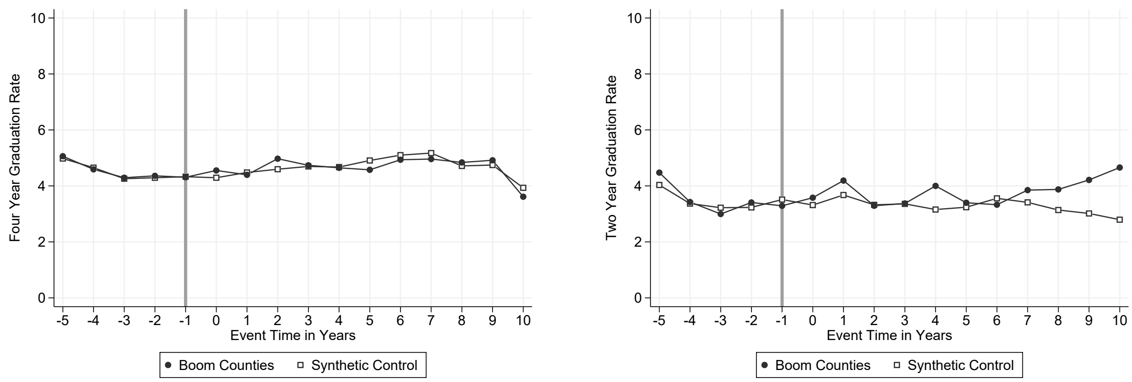
(b) Gap Between Boom Counties and their Synthetic Control



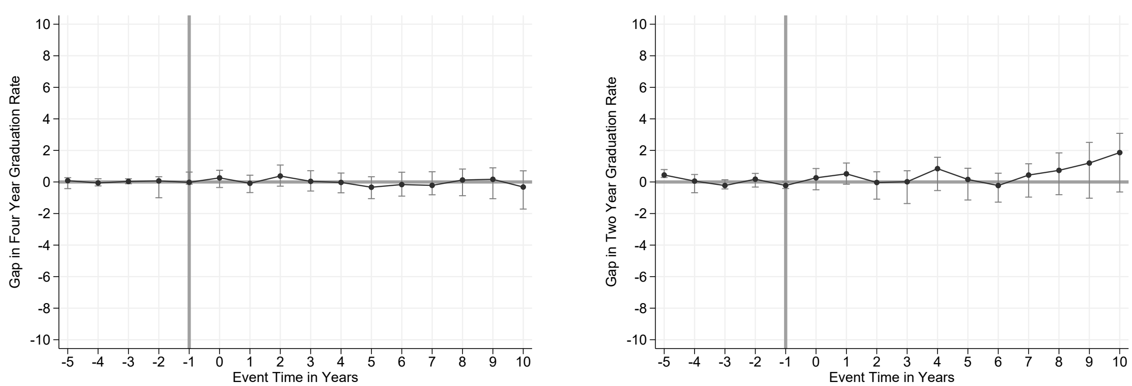
Notes: Panel (a) shows trends in female college enrollment rates by level of institution in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on female college enrollment rates, $\bar{\alpha}_l$, measured in percentage points, from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data source: 2000-2016 Integrated Postsecondary Education Data System (IPEDS).

Figure 1.15: Male College Graduation Rates by Level of Institution

(a) Trends



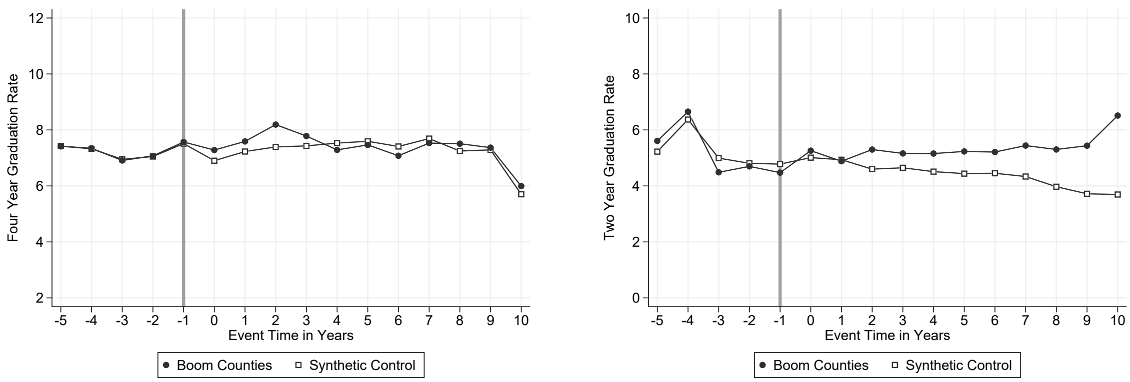
(b) Gap Between Boom Counties and their Synthetic Control



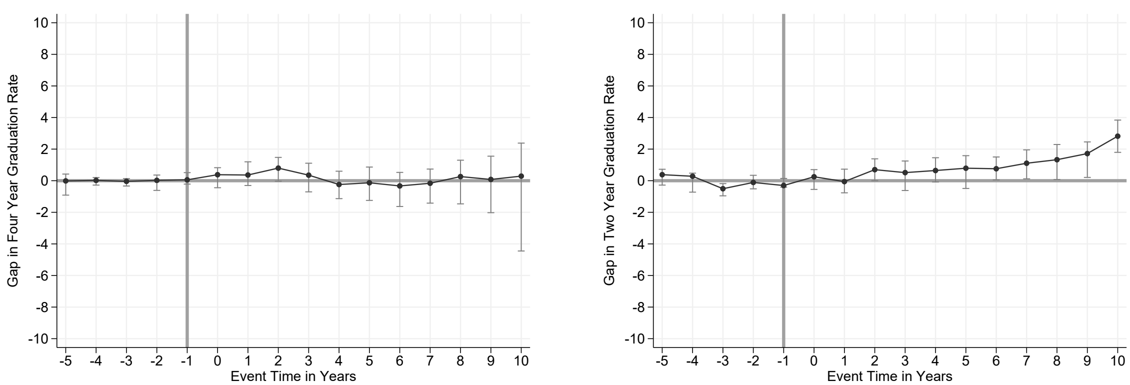
Notes: Panel (a) shows trends in male college graduation rates by level of institution in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on male college graduation rates, $\bar{\alpha}_l$, measured in percentage points, from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data source: 2000-2016 Integrated Postsecondary Education Data System (IPEDS).

Figure 1.16: Female College Graduation Rates by Level of Institution

(a) Trends



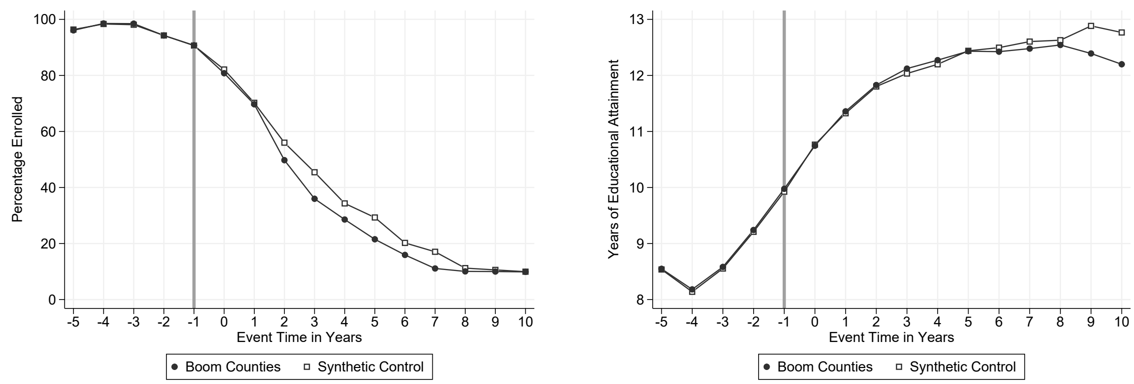
(b) Gap Between Boom Counties and their Synthetic Control



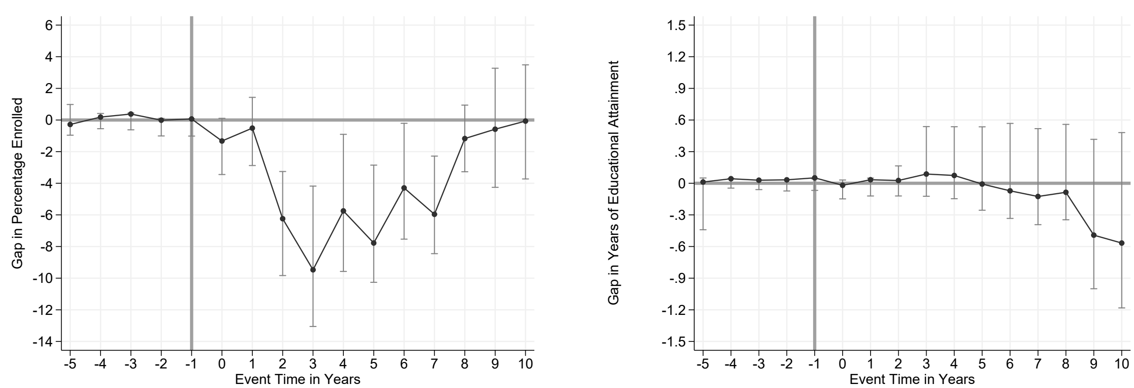
Notes: Panel (a) shows trends in female college graduation rates by level of institution in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on female college graduation rates, $\bar{\alpha}_l$, measured in percentage points, from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data source: 2000-2016 Integrated Postsecondary Education Data System (IPEDS).

Figure 1.17: College Enrollment Rates and Educational Attainment of Males Aged 16 to 19 at the Start of a Boom

(a) Trends



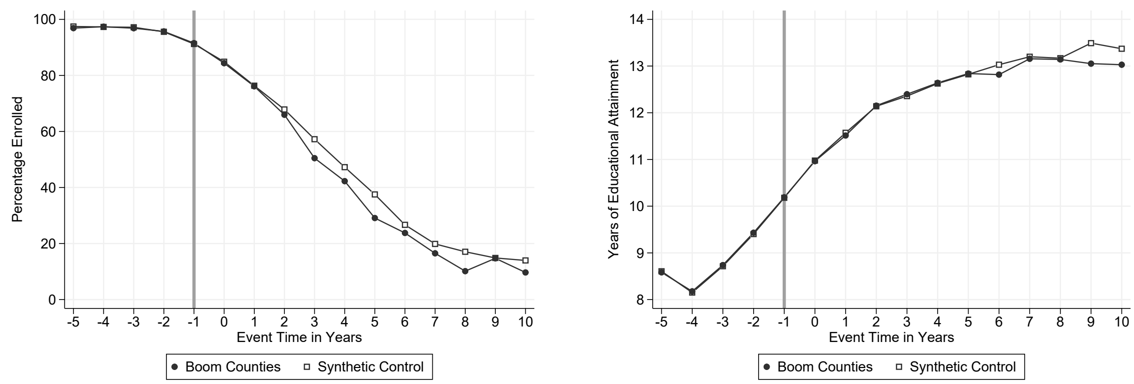
(b) Gap Between Boom Counties and their Synthetic Control



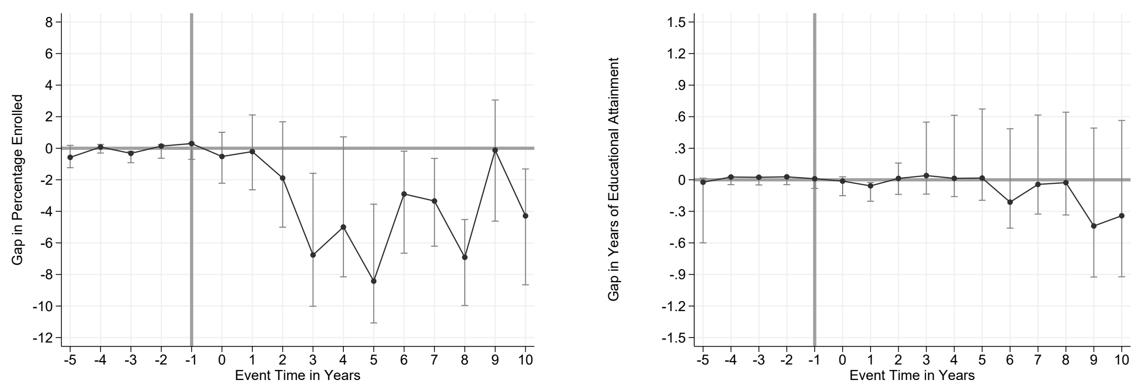
Notes: Panel (a) shows trends in the proportion of men aged 16 to 19 at the start of a boom enrolled in college, as well as their educational attainment, in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects, $\bar{\alpha}_l$ from equation (1.12), of fracking on the proportion of these men enrolled in college, measured in percentage points, as well as on their educational attainment, measured in years of completed schooling. For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data source: 2005-2017 American Community Survey (ACS).

Figure 1.18: College Enrollment Rates and Educational Attainment of Females Aged 16 to 19 at the Start of a Boom

(a) Trends



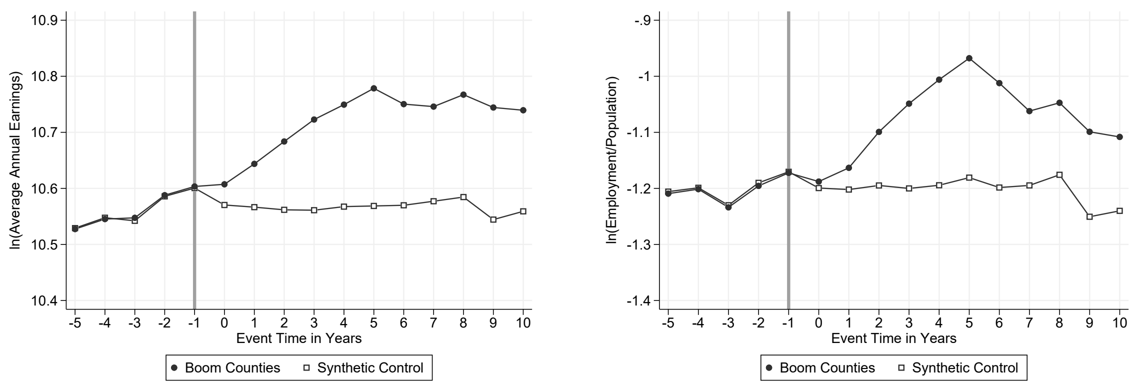
(b) Gap Between Boom Counties and their Synthetic Control



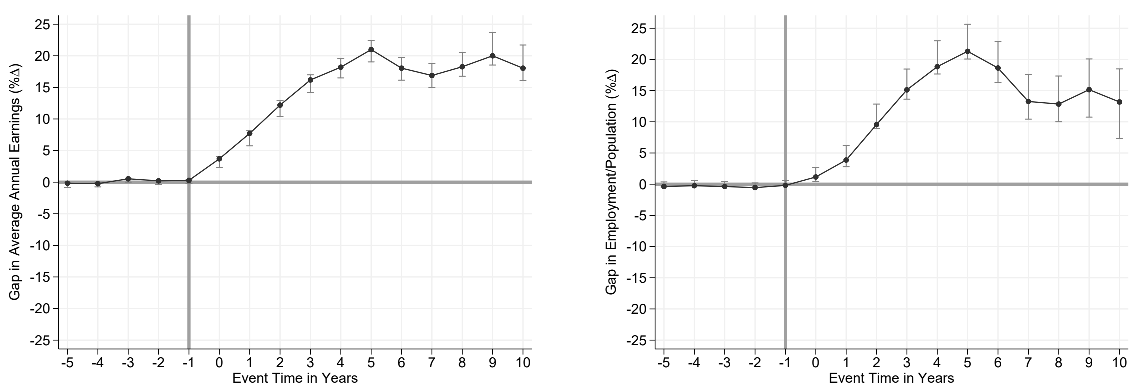
Notes: Panel (a) shows trends in the proportion of women aged 16 to 19 at the start of a boom enrolled in college, as well as their educational attainment, in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects, $\bar{\alpha}_l$ from equation (1.12), of fracking on the proportion of these women enrolled in college, measured in percentage points, as well as on their educational attainment, measured in years of completed schooling. For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data source: 2005-2017 American Community Survey (ACS).

Figure 1.19: Average Earnings and Employment-to-Population Ratio of All Males

(a) Trends



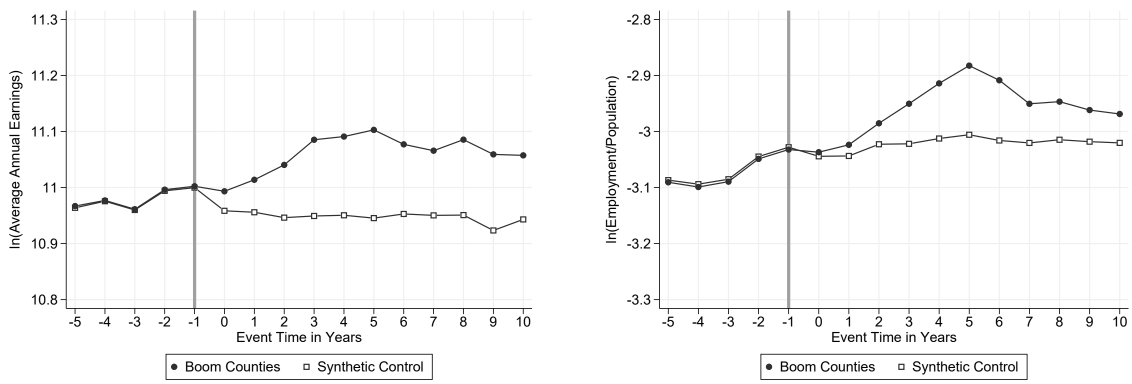
(b) Gap Between Boom Counties and their Synthetic Control



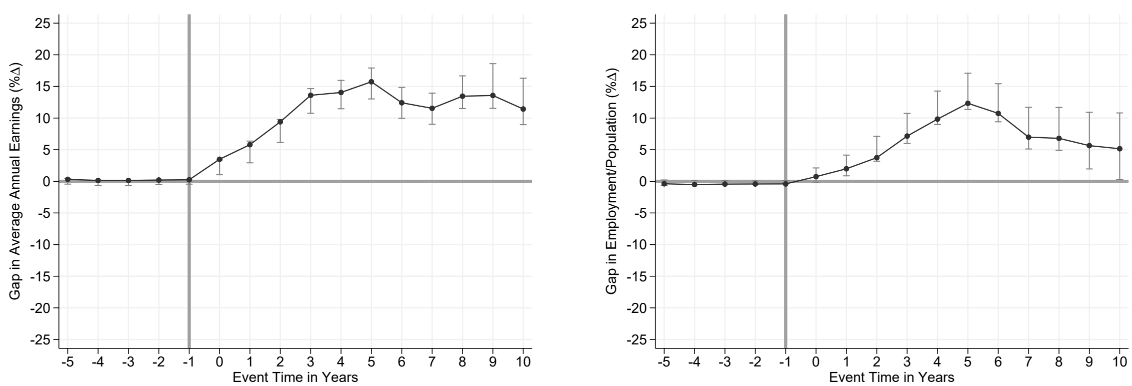
Notes: Panel (a) shows trends in the natural log of male average annual earnings and the natural log of the male employment-to-population ratio in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on male average annual earnings and the male employment-to-population ratio, $\bar{\alpha}_l$, measured in percentage changes, from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data Source: 2000-2016 Quarterly Workforce Indicators.

Figure 1.20: Average Earnings and Employment-to-Population Ratio of College-Educated Males

(a) Trends



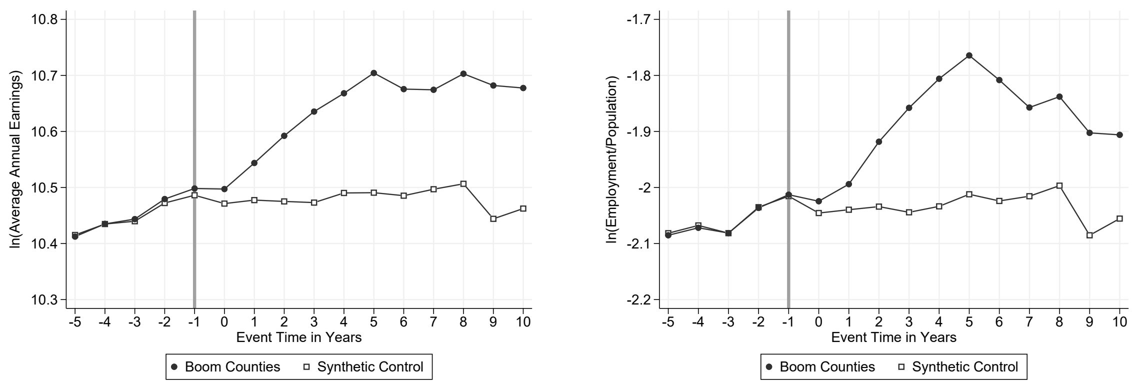
(b) Gap Between Boom Counties and their Synthetic Control



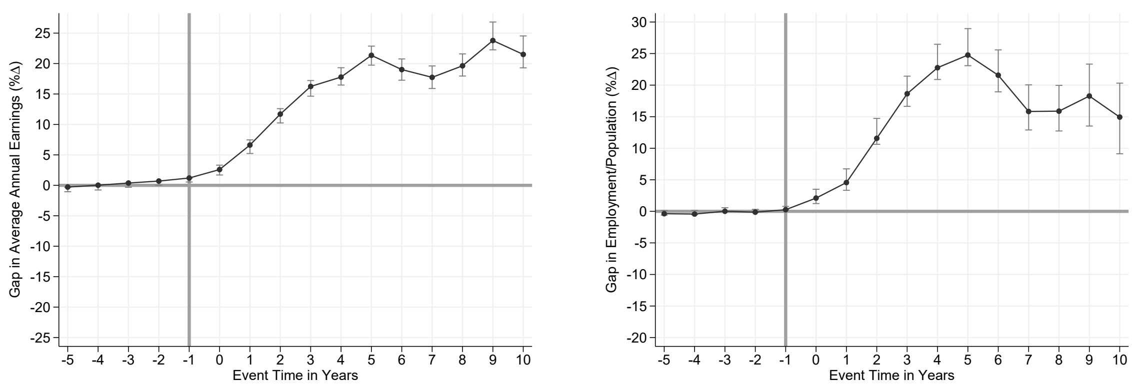
Notes: Panel (a) shows trends in the natural log of college-educated male average annual earnings and the natural log of the college-educated male employment-to-population ratio in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on college-educated male average annual earnings and the college-educated male employment-to-population ratio, $\bar{\alpha}_l$, measured in percentage changes, from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data Source: 2000-2016 Quarterly Workforce Indicators.

Figure 1.21: Average Earnings and Employment-to-Population Ratio of Non-College-Educated Males

(a) Trends



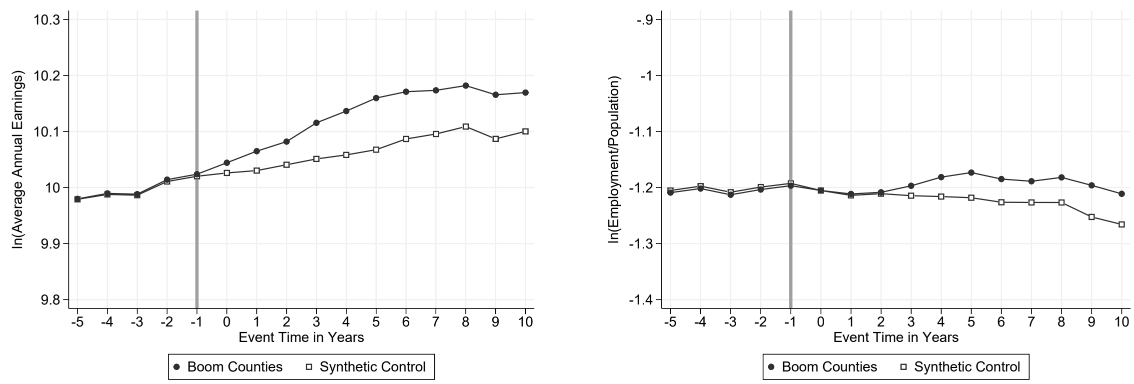
(b) Gap Between Boom Counties and their Synthetic Control



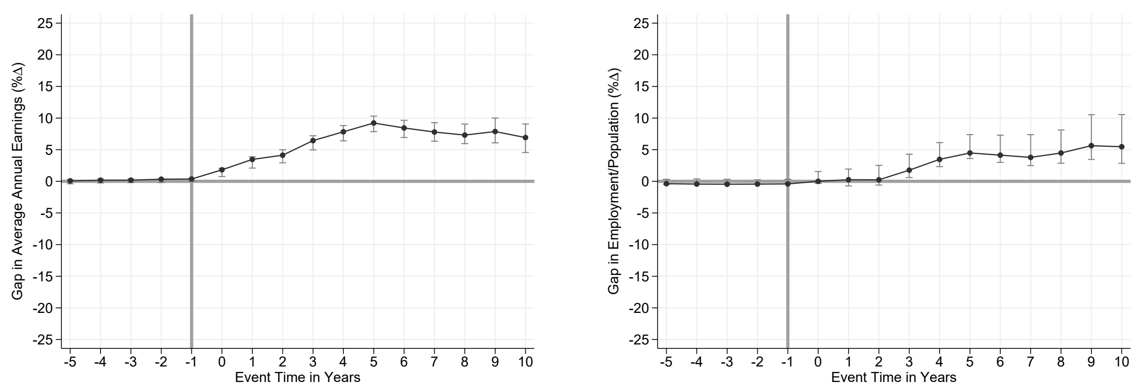
Notes: Panel (a) shows trends in the natural log of non-college-educated male average annual earnings and the natural log of the non-college-educated male employment-to-population ratio in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on non-college-educated male average annual earnings and the non-college-educated male employment-to-population ratio, $\bar{\alpha}_l$, measured in percentage changes, from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data Source: 2000-2016 Quarterly Workforce Indicators.

Figure 1.22: Average Earnings and Employment-to-Population Ratio of All Females

(a) Trends



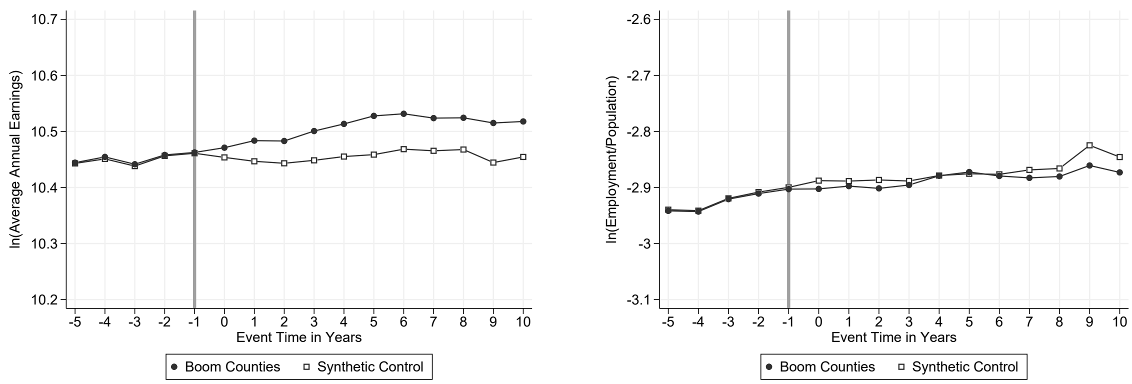
(b) Gap Between Boom Counties and their Synthetic Control



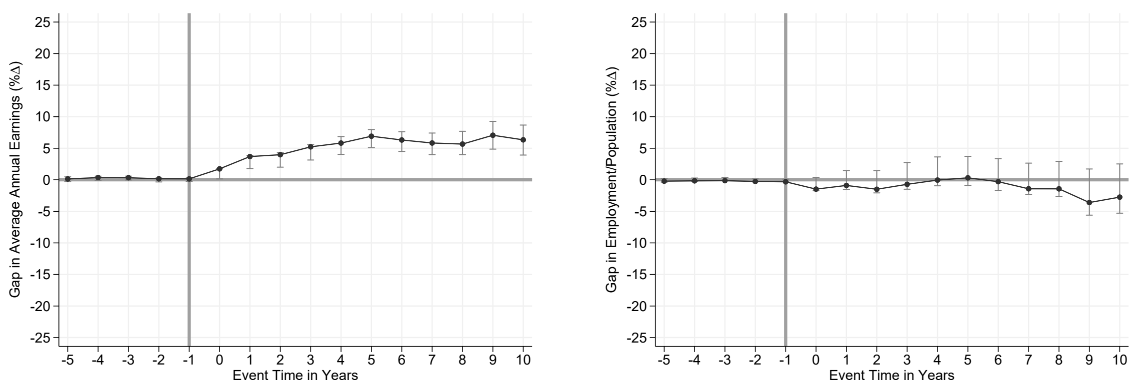
Notes: Panel (a) shows trends in the natural log of female average annual earnings and the natural log of the female employment-to-population ratio in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on female average annual earnings and the female employment-to-population ratio, $\bar{\alpha}_l$, measured in percentage changes, from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data Source: 2000-2016 Quarterly Workforce Indicators.

Figure 1.23: Average Earnings and Employment-to-Population Ratio of College-Educated Females

(a) Trends



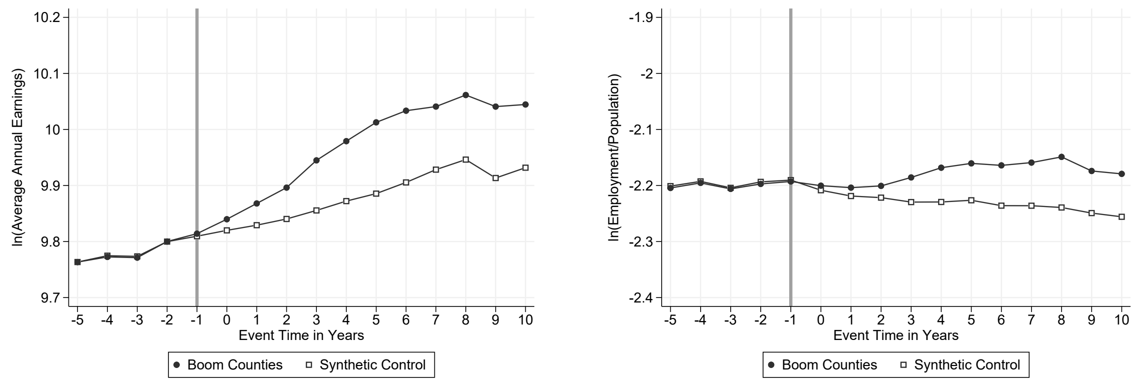
(b) Gap Between Boom Counties and their Synthetic Control



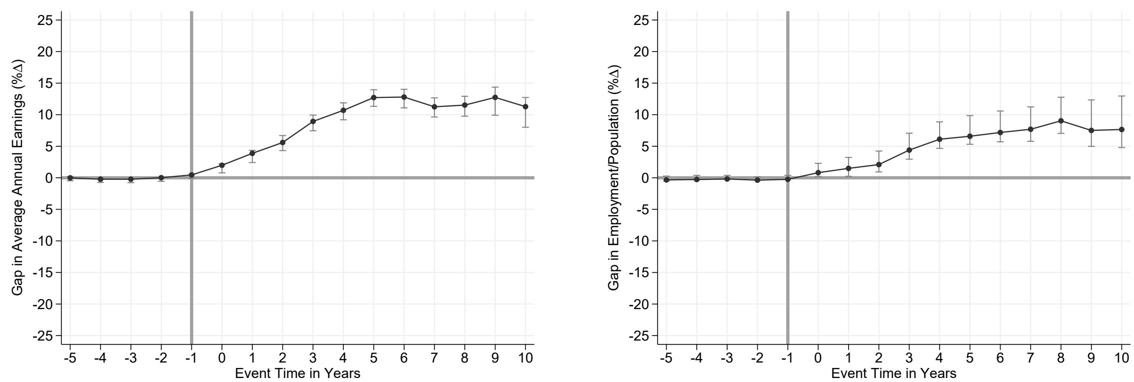
Notes: Panel (a) shows trends in the natural log of college-educated female average annual earnings and the natural log of the college-educated female employment-to-population ratio in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on college-educated female average annual earnings and the college-educated female employment-to-population ratio, $\bar{\alpha}_l$, measured in percentage changes, from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data Source: 2000-2016 Quarterly Workforce Indicators.

Figure 1.24: Average Earnings and Employment-to-Population Ratio of Non-College-Educated Females

(a) Trends



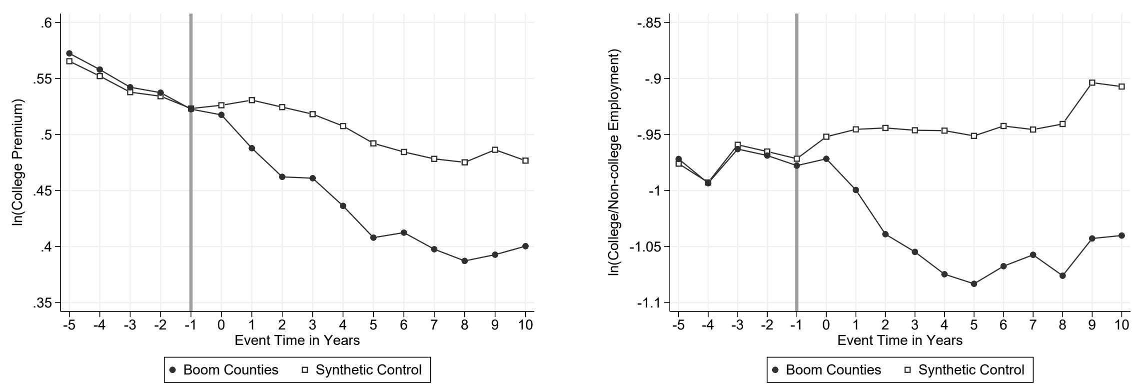
(b) Gap Between Boom Counties and their Synthetic Control



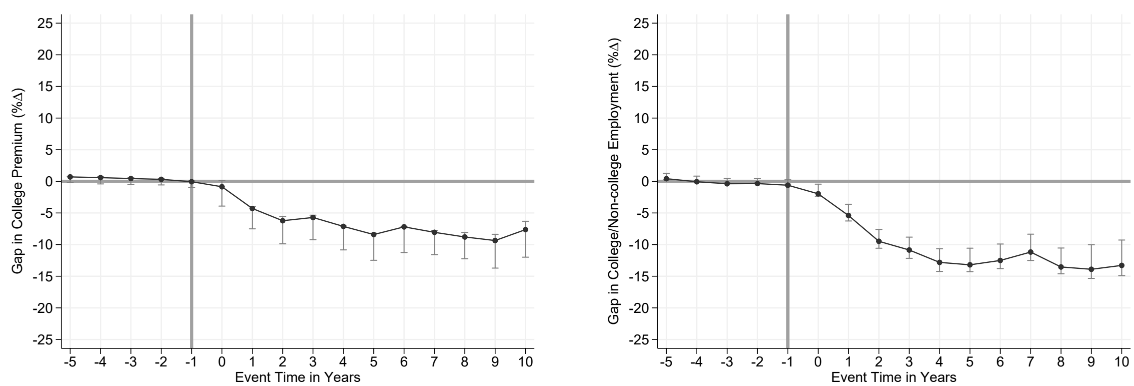
Notes: Panel (a) shows trends in the natural log of non-college-educated female average annual earnings and the natural log of the non-college-educated female employment-to-population ratio in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on non-college-educated female average annual earnings and the non-college-educated female employment-to-population ratio, $\bar{\alpha}_l$, measured in percentage changes, from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data Source: 2000-2016 Quarterly Workforce Indicators.

Figure 1.25: Male College Premium and College-to-Non-College-Educated Employment Ratio

(a) Trends



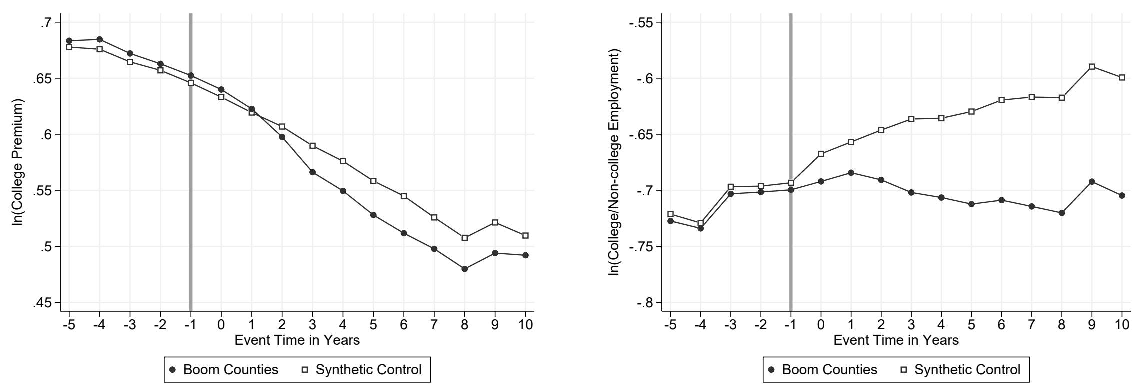
(b) Gap Between Boom Counties and their Synthetic Control



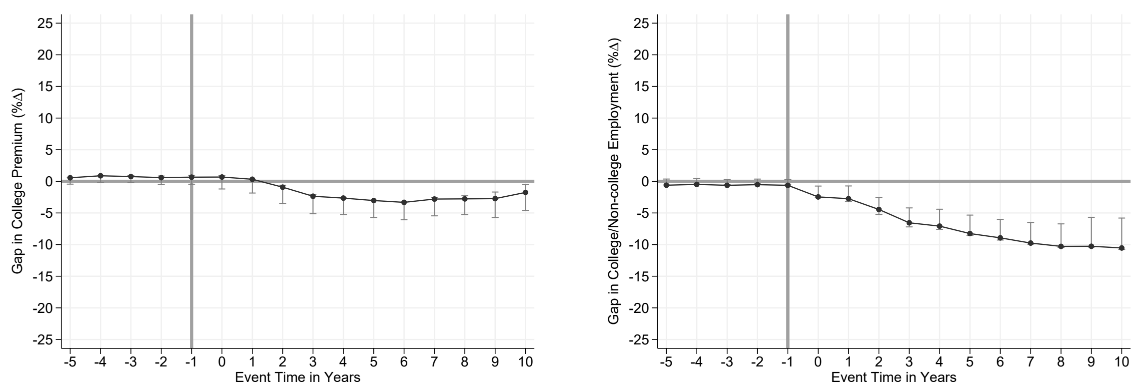
Notes: Panel (a) shows trends in the natural log of the male college premium and the natural log of the male college-to-non-college employment ratio in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on the male college premium and college-to-non-college-educated employment ratio, $\bar{\alpha}_l$, measured in percentage changes, from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data Source: 2000-2016 Quarterly Workforce Indicators.

Figure 1.26: Female College Premium and College-to-Non-College-Educated Employment Ratio

(a) Trends



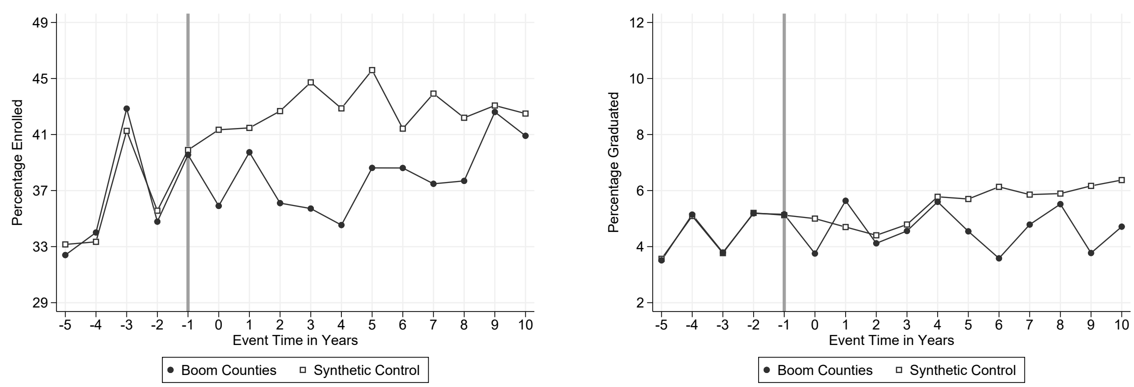
(b) Gap Between Boom Counties and their Synthetic Control



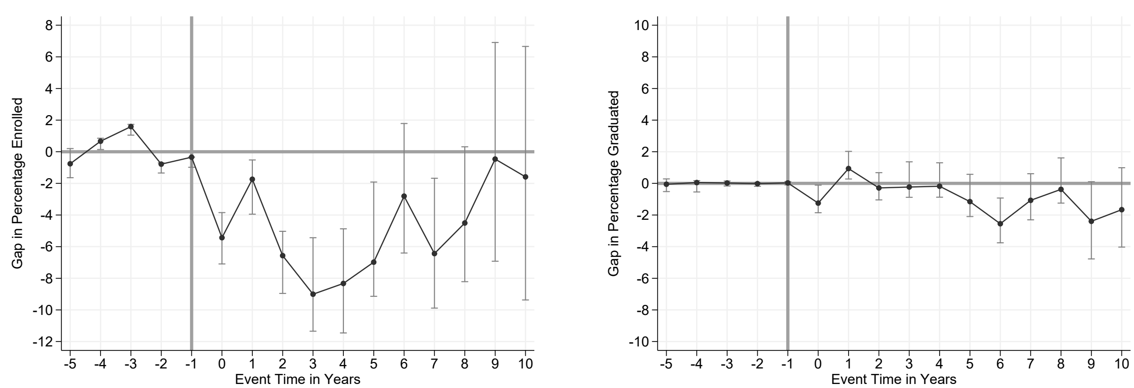
Notes: Panel (a) shows trends in the natural log of the female college premium and the natural log of the female college-to-non-college employment ratio in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on the female college premium and college-to-non-college-educated employment ratio, $\bar{\alpha}_l$, measured in percentage changes, from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data Source: 2000-2016 Quarterly Workforce Indicators.

Figure 1.27: College Enrollment and Graduation Rates of Long-Term Resident Males Aged 18 to 26

(a) Trends



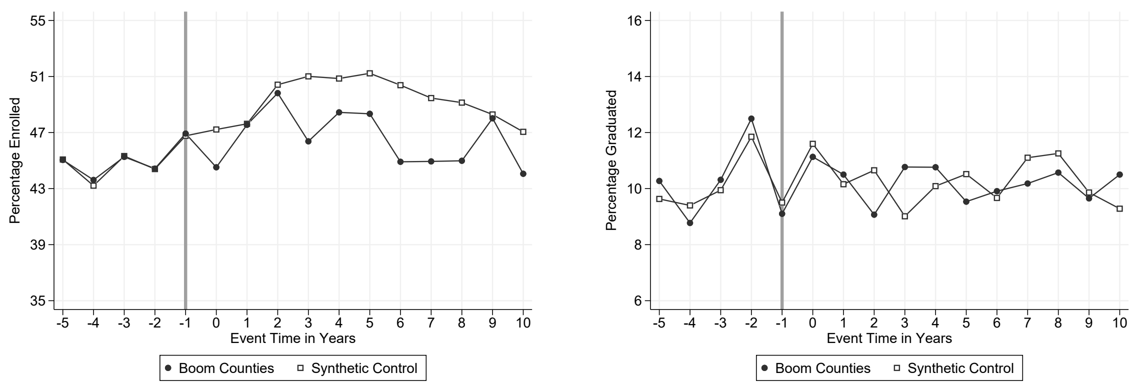
(b) Gap Between Boom Counties and their Synthetic Control



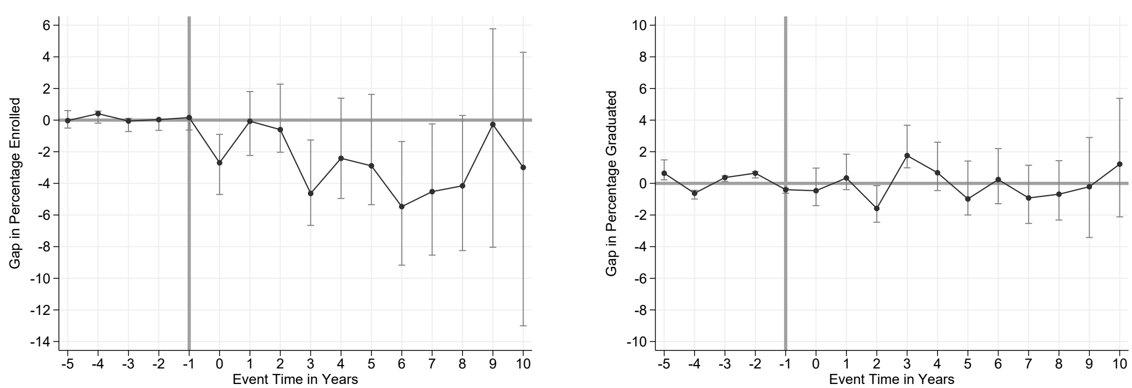
Notes: Panel (a) shows trends in the proportion of long-term resident men enrolled in and graduated from college in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on the proportion of long-term resident men enrolled in and graduated from college, $\bar{\alpha}_l$, measured in percentage points (p.p.), from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data source: 2005-2017 American Community Survey (ACS).

Figure 1.28: College Enrollment and Graduation Rates of Long-Term Resident Females Aged 18 to 26

(a) Trends



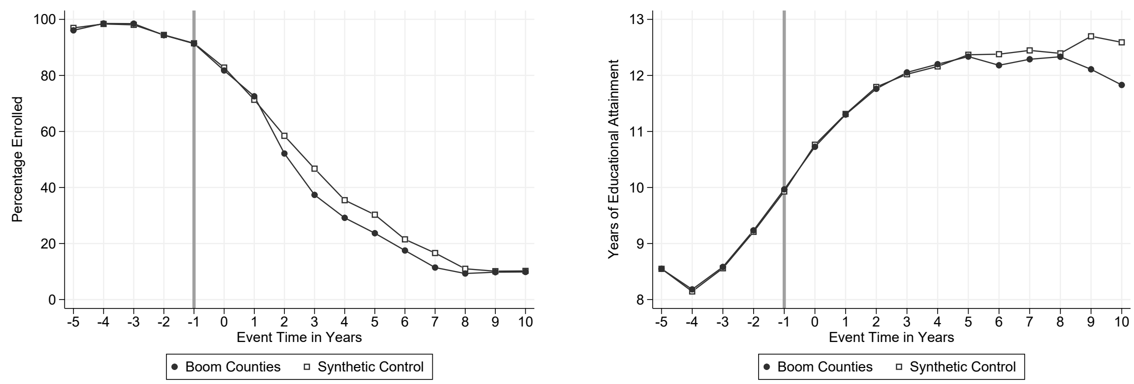
(b) Gap Between Boom Counties and their Synthetic Control



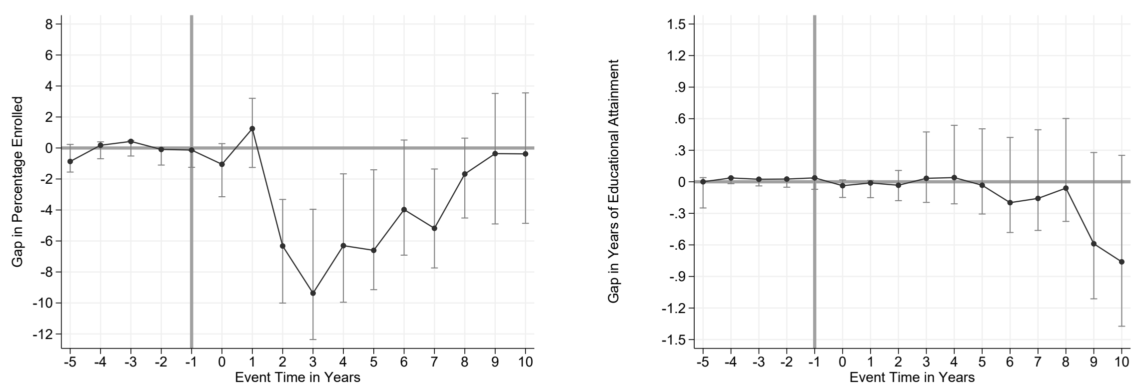
Notes: Panel (a) shows trends in the proportion of long-term resident women enrolled in and graduated from college in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects of fracking on the proportion of long-term resident women enrolled in and graduated from college, $\bar{\alpha}_l$, measured in percentage points (p.p.), from equation (1.12). For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data source: 2005-2017 American Community Survey (ACS).

Figure 1.29: College Enrollment Rates and Educational Attainment of Long-Term Resident Males Aged 16 to 19 at the Start of a Boom

(a) Trends



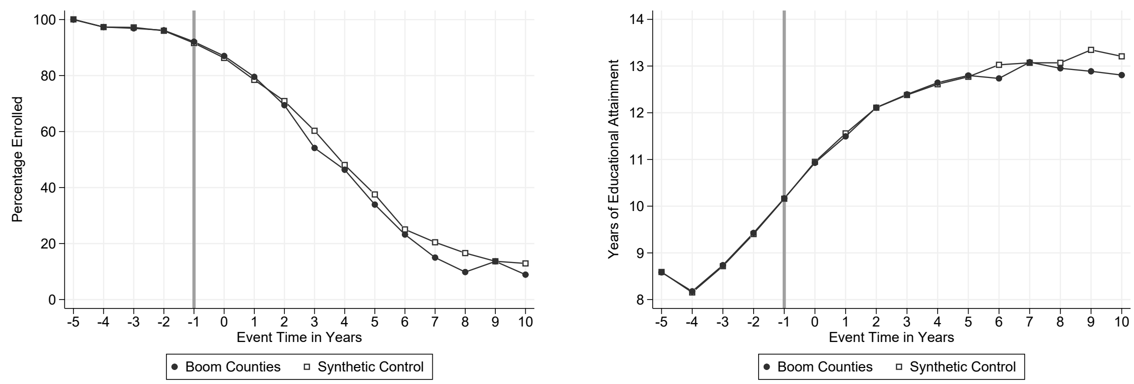
(b) Gap Between Boom Counties and their Synthetic Control



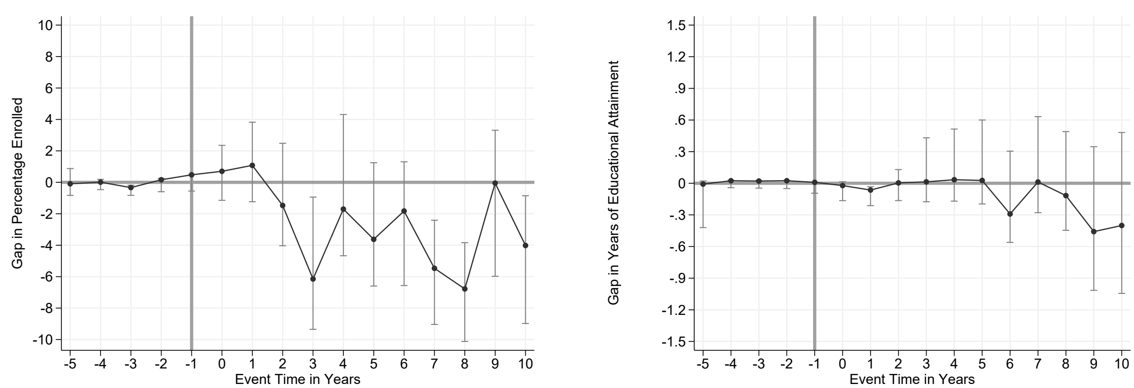
Notes: Panel (a) shows trends in the proportion of long-term resident men aged 16 to 19 at the start of a boom enrolled in college, as well as their educational attainment, in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects, $\bar{\alpha}_l$ from equation (1.12), of fracking on the proportion of these men enrolled in college, measured in percentage points, as well as on their educational attainment, measured in years of completed schooling. For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data source: 2005-2017 American Community Survey (ACS).

Figure 1.30: College Enrollment Rates and Educational Attainment of Long-Term Resident Females Aged 16 to 19 at the Start of a Boom

(a) Trends



(b) Gap Between Boom Counties and their Synthetic Control



Notes: Panel (a) shows trends in the proportion of long-term resident women aged 16 to 19 at the start of a boom enrolled in college, as well as their educational attainment, in boom counties and their synthetic controls. Panel (b) shows average lead and lag specific effects, $\bar{\alpha}_l$ from equation (1.12), of fracking on the proportion of these women enrolled in college, measured in percentage points, as well as on their educational attainment, measured in years of completed schooling. For each lead and lag specific effect, the 95 percent confidence intervals are also shown, and are estimated using equation (1.14) following the steps outlined in section 1.5.3. Data source: 2005-2017 American Community Survey (ACS).

Chapter 2

Geographic Dispersion of Economic Shocks and College Attainment

2.1 Introduction

Improved drilling and production technologies have fundamentally changed the oil and gas industry in the United States. Hydraulic fracturing ("fracking") and horizontal drilling have made it economically feasible to extract oil and gas from low permeability rocks and tightly packed sands. As a result, many counties sitting on shale formations have experienced large and sudden increases in the production of fracked oil and gas. Shale energy output requires considerable initial cost and effort, and unlike conventional oil and gas extraction, declines steeply during the first few years of production. Large increases in production coupled with the need to continually drill new wells has resulted in local labor demand shocks within these counties, particularly for non-college-educated men (Cascio and Narayan, 2017; Kearney and Wilson, 2018; Neilson, 2020).

By increasing the opportunity cost of additional years of schooling, new fracking production has been shown to reduce educational outcomes within areas that produce fracked oil and gas (Cascio and Narayan, 2017; Niekamp, 2019; Rickman et al., 2017; Neilson, 2020; Weber, 2014). Figure 2.1 shows for counties that experienced a boom in fracking production, the percentage change in average enrollments at four-year institutions, as well as the value of new fracking production, both measured relative to the year in which the boom started in each county.[1] While fracking production expands and peaks in these counties, enrollments decrease. As fracking production contracts in these counties, enrollments increase and even surpass their levels nine to ten years earlier when the boom started.

It is likely that not all individuals responding to these labor demand shocks live and shop in the counties where the new extraction is taking place. This will result in spillovers of economic activity in neighboring counties that would not be picked up in a county-level analysis. If the costs and returns to additional years of schooling are also changing in areas near new fracking production, economic theory would predict that marginal decisions related to college attendance would be altered by these changing incentives in areas surrounding fracking production, even in areas that do not produce oil and gas themselves. Moreover, it is not uncommon to attend college outside of your county of residence. Consequently, Institutions that are near fracking counties might see a reduction in the demand for their education from prospective students within nearby fracking counties as a result of new fracking production.

For policy evaluation and impact analysis, it is important to understand the spatial dispersion of economic shocks such as these. The purpose of this paper is to evaluate whether and to what extent new fracking production occurring at varying distances from a county affects labor market and educational outcomes within that county. I evaluate these spillover effects by first finding the value of new fracking production in concentric 20-mile doughnuts around all U.S. counties. I then estimate how fracking production at varying distances from a county affects earnings, employment, and college enrollment within that county, controlling for the production of neighboring counties.

I find that one million dollars of new fracking production within a county generates $68,000 in new earnings, 1.03 new jobs, and a reduction of 0.04 enrollments within that county. One million dollars of new production within 40 miles of a county (not including own-county production, if any) generates $11,100, 0.11 new jobs, and a reduction of 0.02 enrollments within that county. New production between 40 and 60 miles away has a modest but significant effect on earnings and employment, but no significant effect on enrollment. I find little evidence that new fracking production farther than 60 miles from a county affects earnings, employment, or enrollment. The own-county and spillover effects of new production on earnings and employment are larger in

[1] The identification of boom counties and the year in which the boom started in each county come from Neilson (2020).

magnitude for men relative to women, and for individuals with an education of high school or less relative to those with some college or college graduates. I document that between 2006 and 2016, over $355 billion worth of fracked oil and gas was produced. The vast majority of this production (about 78 percent) came from the top 10 percent of producing counties.

This study is related to two growing literatures. First, building on insights from existing models (Becker, 1964; Mincer, 1958), researchers have shown that labor market conditions affect college attainment (Betts and McFarland, 1995; Black et al., 2005; Atkin, 2016; Cascio and Narayan, 2017; Charles et al., 2018; Neilson, 2020). I extend this literature by examining the spatial propagation of the effects of shocks to local labor market conditions on college enrollment. Second, while there is a general consensus in the literature on the effects of the fracking boom that labor market opportunities are greatly enhanced in areas that engage in fracking (Bartik et al., 2019; Cascio and Narayan, 2017; Feyrer et al., 2017; Kearney and Wilson, 2018; Krupnick and Echarte, 2017; Maniloff and Mastromonaco, 2017; Neilson, 2020; Weber, 2014), less is known about how spatially spread out the effects are. I contribute to this literature by estimating spillover effects of fracking on earnings and employment by gender and educational attainment. This is important because it informs us on how the costs and returns to education are affected by fracking-induced labor demand shocks, even within counties that do not engage in fracking.

In the next section I offer a brief background on fracking production and description of my data. Section 2.3 explains my empirical strategy. In Section 2.4, I present my results and provide a brief discussion of their implications. Section 2.5 concludes.

2.2 Context and Data

Shale oil and gas refers to fossil fuels that are trapped between layers of shale rock far below the surface of the earth. Extracting these fossil fuels involves drilling horizontally into the rock formation and injecting a fluid at a high enough pressure to create and maintain fractures through which the trapped resources can escape. According to Wang and Krupnick (2015), there were a number of factors that converged in the early 2000s that made it profitable for firms to produce large quantities of shale gas, but the most important factor was innovations in technology.[2] Three areas of

[2]Other factors they suggest include high natural gas and oil prices, government policy, private entrepreneurship, private land and mineral rights ownership, market structure, favorable geology, water availability, and natural gas pipeline infrastructure.

innovation, horizontal drilling, hydraulic fracturing, and three-dimensional seismic imaging, were of particular importance in making it cost-effective to produce shale oil and gas.[3]

As a result of these innovations in the early 2000s, there has been a marked shift in the way in which oil and gas is produced in the United States. Figure 2.2 shows the aggregate annual level of oil and gas production by drill type of wells that began producing either oil or gas in the year 2000 or later. Production from traditional vertically drilled wells exceeded that of non-vertically drilled (fracked) wells in the early 2000s and remained relatively constant between 2000 and 2017. Around the end of the 2000s however, shale oil and gas production increased immensely across the United States, with the production of fracked oil and gas from these new wells far surpassing production from traditional vertically drilled wells. Around the height of production in 2017, almost 2 billion barrels of oil and 20 trillion cubic feet of natural gas were being produced by fracking. To contextualize these amounts, in 2017, about 7.3 billion barrels of oil and 27.1 trillion cubic feet of natural gas were consumed in total in the United States (US Energy Information Administration, 2020).

The ability to produce fracked oil and gas depends critically on the availability of the resource, making the locations of this large increase in fracking production geographically specific. For each county, Figure 2.3 shows the cumulative value of fracked oil and gas production between 2007 and 2016. Fracking production was most heavily concentrated in Texas, North Dakota, Wyoming, Pennsylvania, Louisiana, and Oklahoma. There is considerable variation in the value of fracked oil and gas across counties, even within these states. Figure 2.3 also shows which counties have at least one institution of higher education. A large number of counties with institutions of higher education either have fracking production within the county, or are near a county that engages in fracking. This is even true in more rural areas where fracking is heavily concentrated, like West Texas and North Dakota, for example.

2.2.1 Data Description

Information on local level oil and gas production comes from Enverus (formerly Drillinginfo), a private company that collects data on the monthly production amount, initial drill date, drilling

[3]Horizontal drilling into the rock formations made it possible for firms to access thousands of feet of trapped fossil fuels, as opposed to vertically drilling down into one area. Hydraulic fracturing, including the experimentation with the composition of the fracking fluid, made possible the release of the oil and gas once the well was horizontally drilled. Three-dimensional seismic imaging aided in more precise identification of these formations and their geology.

direction (vertical or non-vertical), and the location of all wells drilled in the United States.[4] My sample consists of monthly production of oil, measured in barrels, and gas, measured in thousands of cubic feet, on properties that began producing either oil, gas, or oil and gas after January 1, 2000. For consistency across samples, I aggregate all fracked oil and gas produced to the county-year level. To convert oil and gas production into comparable dollar amounts, I use average annual national prices for oil and gas, recorded by the Energy Information Administration (EIA), and create a measure of the value of fracked oil and gas produced in a given county and year.[5] All dollar amounts in this study are converted into 2010 dollars using the Consumer Price Index.

My source of information on enrollment in institutions of higher education comes from the Integrated Postsecondary Education Data System (IPEDS). IPEDS is a system of interrelated survey components conducted annually by the National Center for Education Statistics. IPEDS gathers information, including a measure of first-time, full-year enrollments, from every college, university, and technical/vocational institution that participates in the federal student financial aid programs in the United States. I match these colleges, universities, and technical/vocational institutions to counties, and compute estimates of first-time, full-year enrollments by gender and institution level in each county and each year between 2006 and 2016.[6]

Data on earnings and employment come from the Quarterly Workforce Indicators (QWI). The source data for the QWI is the Longitudinal Employer-Household Dynamics (LEHD) linked employer-employee microdata, covering over 95% of United States private sector jobs. The QWI provide local labor market statistics at the county level by industry and worker demographics, such as worker age, gender, educational attainment, and race/ethnicity. Due to confidentiality protection obligations, these data are only made available in certain two-way worker characteristic

[4]The use of these data was provided by Enverus through an academic use agreement. Similar to Feyrer et al. (2017) and Kearney and Wilson (2018), I consider all oil and gas produced from non-vertical wells as fracked oil and gas.

[5]For natural gas, I use the reported average annual citygate prices, which represent the total cost paid by gas distribution companies for gas received at the point where the gas is physically transferred from a pipeline company or transmission system. This price is intended to reflect all charges for the acquisition, storage, and transportation of gas as well as other charges associated with the local distribution company's obtaining the gas for sale to consumers. For crude oil, I use the reported West Texas Intermediate (WTI) average annual price. Prices of WTI are often listed in oil price reports, alongside other important oil markers, like UK Brent or the OPEC basket.

[6]IPEDS classifies the level of institution as either four-year (or higher), two-but-less-than-four-year, or less-than-two-year. Four-year or higher institutions are institutions that offer programs of at least four years duration or ones that offer programs at or above the baccalaureate level. Four-year institutions include schools that offer postbaccalaureate certificates only or those that offer graduate programs only, and also include free-standing medical, law or other first-professional schools. Two-year instituions offer programs of at least two but less than four years duration. Two-year institutions include occupational and vocational schools with programs of at least 1800 hours and academic institutions with programs of less than four years (not including bachelor's degree-granting institutions where the baccalaureate program can be completed in three years). Less than two-year institutions are postsecondary institutions that offer programs of less than two-year duration below the baccalaureeate level, including occupational and vocational schools with programs that do not exceed 1800 clock hours.

crossings.[7] For each industry in a county, I have measures of total annual earnings and employment counts by gender and educational attainment of workers.

2.3 Empirical Strategy

To evaluate the labor market and educational effects of new fracking activity, it is important to consider the way in which the intensity of that activity is measured. I capture variation in fracking-related economic activity by considering the value of oil and gas production from all new wells that began drilling in the current year. Using the value of *new* production is important because the extraction of unconventional oil and gas through fracking requires considerable initial effort to prepare the drill site, perform the actual drilling and fracking, and otherwise prepare the necessary infrastructure for production. Moreover, unlike conventional oil and gas extraction, shale energy output declines steeply during the first few years of production (Considine et al., 2011). As a result, operators must be continually drilling new wells.

I scale my measure of new fracking production, as well as my outcome variables, by the one-year lag in total employment.[8] Scaling all variables by the one-year lag of total employment allows small and large population areas to be handled consistently in the same regression. My outcome variables, $Y_{i,t}$, consist of per capita annual earnings, employment, and enrollment.[9] My main independent variable, $NewProd_{i,t}$, is the total value of fracked oil and gas production from new wells that began drilling in the current year, measured in million dollars per capita.

In the QWI, workers at job sites are reported as employed in the county from which they are supervised. If physical wells are located in a different county than their supervisory unit, this may understate the impact of new oil and gas production in the county where the well is located. In addition, workers may not live and shop in the counties where the new extraction is taking place. This will result in spillovers of economic activity in neighboring counties that will not be picked up in a county-level analysis.

[7]The available worker characteristic crossings are gender and educational attainment, gender and age, and race and ethnicity of workers.

[8]One million dollars of new production will have a different impact in a relatively small population county, compared to the same amount of new production in a relatively large population county.

[9]All references to per capita refer to scaling by the lagged level of total employment (James and Smith, 2019; Feyrer et al., 2017).

2.3.1 Econometric Model

There are various ways to analyze the extent of these spillovers. For example, Feyrer et al. (2017) regress the change in wage income and employment on the value of new fracking production at arbitrary distances from new production. This involves aggregating all variables within a fixed radius of the centroid of a county, and scaling each by the relevant one-year lag of total employment. They argue that the changes in these estimates with increasing distance show the geographic dispersion of the effect of new production on income and employment as we get farther from the sources of the new production.

James and Smith (2019) point out that this methodology may be problematic since oil producing counties are spatially correlated. Because oil producing counties tend to be clustered over shale plays, producing counties likely experience inward spillover effects from neighboring producing counties. If these inward spillovers are not accounted for, they would result in a positive correlation between the error term and own-county production that causes an upward bias in the estimated effect of own-county production on own-county income and employment. Aggregating across space may not resolve this issue since even clusters of oil producing counties are likely to be located near additional oil production (see Figure 2.3).

James and Smith (2019) build on Feyrer et al. (2017) and propose an alternative estimation strategy that conditions the effect of own-county production on neighboring production. Similar to James and Smith (2019), I first find the value of new production per capita in concentric 20-mile doughnuts around all U.S. counties. I then evaluate how fracking production at varying distances from a county affects earnings, employment, and enrollment within that county, controlling for the production of neighboring counties. Specifically, I estimate

$$\Delta Y_{i,t} = \sum_{d=1}^{d=10} \left[\gamma_d \times NewProd_{d,i,t} + \lambda_d \times NewProd_{d,i,t-1} \right] + \phi_i + \delta_t + \varepsilon_{i,t} \, , \tag{2.1}$$

where $\Delta Y_{i,t}$ is the one-year change in annual earnings, employment, or enrollment per capita in county i in year t, $NewProd_{d,i,t}$ is the value of new production per capita in doughnut d away from county i, and ϕ_i and δ_t are county and year fixed effects, respectively.[10] $d = 1$ corresponds to production within county i; $d = 2$ corresponds to production within a doughnut that has an

[10]Similar to James and Smith (2019), all dependent and independent variables are scaled by the one-year lag of employment in county i.

exterior radius of 40 miles, not including production in county i; $d = 3$ corresponds to production within a doughnut that has an exterior radius of 60 miles, and an interior radius of 40 miles; and so on in 20 mile increments up to an exterior radius of 200 miles.[11] The parameters of interest are the γ_d, which capture the contemporaneous effects of new production at various distances from county i on $Y_{i,t}$. For example, γ_1 represents the amount of new (or lost) earnings, jobs, or enrollments as a result of one million dollars of new fracking production within county i, controlling for neighboring fracking production. The earnings, employment, or enrollment effects of production within a 40 mile radius are given by γ_2, between 40 and 60 miles away by γ_3, and so on. The one-year lags of new production at the various distances are included to control for potential dynamic effects.

2.3.2 Instrumental Variable - *Potential* New Production

The decision to frack depends on the availability of the resource, but it could also depend on other cultural, economic, or regulatory factors. While the former is exogenous, the latter may not be. For example, some areas may be fracked earlier because land values are relatively low, wages are low, or unemployment is high. For this reason, I follow Feyrer et al. (2017) and instrument for new production in each county using their exposure to geological formations – shale plays – where tight oil and gas can be extracted through fracking.[12] I generate a prediction of *potential* aggregate new production for each year in each county by estimating

$$ln(NewProd_{i,t} + 1) = \phi_i + \sum_{\tau=t} \sum_{j=1}^{J} \theta_{\tau,j} I\{county\ i\ over\ play\ j\} \times I\{year = \tau\} + \nu_{i,t}\ . \tag{2.2}$$

I then transform these predictions into a form consistent with the main independent variables,

$$\widehat{NewProd}_{i,t} = exp\left(\hat{\phi}_i + \sum_{\tau=t} \sum_{j=1}^{J} \hat{\theta}_{\tau,j} I\{county\ i\ over\ play\ j\} \times I\{year = \tau\}\right) - 1\ . \tag{2.3}$$

The main explanatory variables in equation (2.2) are a set of interactions between an indicator that equals 1 if the border of county i intersects shale play j and an indicator that equals 1 in year t. This estimates the average impact of being over shale play j on new production and allows this relationship to vary over time as technology and prices change. Thus, the predicted values for

[11] I start with a radius of 40 miles because very few counties are included in doughnuts up to 20 miles. The average county for example, has only .45 counties within 20 miles of its centroid. For the majority of the observations, zero counties would be included.

[12] Kearney and Wilson (2018) use a similar strategy to estimate how the fracking revolution affected family formation. Other papers related to the use of indirect measures of production include Bartik et al. (2019); Considine et al. (2011); Deller and Schreiber (2012); Fetzer (2014); Maniloff and Mastromonaco (2017); Marchand (2012); and Weber (2014)

new production per capita are based on the timing of new production for all the counties within a particular play.[13]

2.4 Results

In this section, I describe my results from estimating equation (2.1). Tables 2.1, 2.2, and 2.3 show estimates of the own-county effects of new fracking production, γ_1, where the one-year change in earnings, employment, and enrollment per capita are the outcome variables in each table, respectively. Figures 2.4 through 2.9 report earnings, employment, and enrollment effects, γ_d, of new fracking production within doughnuts of various radii surrounding each county. After describing the earnings, employment, and earnings effects of new fracking production, I provide a discussion of the results and their implications.

2.4.1 Earnings

Table 2.1 contains estimates of γ_1 from equation (2.1) where the outcome is the one-year change in earnings per capita by gender and educational attainment. Each cell of Table 2.1 represents the coefficient from a different regression. In column (1) of Panel A, OLS estimates suggest that one million dollars of new fracked oil and gas production is associated with an earnings increase of $68,000 within a county, controlling for inward spillover effects from neighboring producing counties. My estimated effect is comparable to those found by Feyrer et al. (2017) and James and Smith (2019). Feyrer et al. (2017) and James and Smith (2019) find that one million dollars of new fracked oil and gas production is associated with a wage increase of $25,000 to $34,000 using wage estimates from the Bureau of Labor Statistics. Using measures of adjusted gross income from the Internal revenue Service, Feyrer et al. (2017) also estimate that one million dollars of new production is associated with an income increase of $81,000 within a county.

Looking separately at the earnings effects of own-county new production by educational attainment, I find that increases in the earnings of individuals with a high school diploma or less account for the largest share of the earnings increase of all individuals, regardless of gender. One million dollars of new fracking production is associated with an earnings increase of about $31,000

[13]Because each individual county represents a small part of the shale play's production, the predicted measure of new production depends little on the roll out of fracking within individual counties.

for workers with a high school diploma or less, $20,000 for workers with some college, and $9,000 for college graduate workers.[14] Comparing panels B and C of Table 2.1, I find that the earnings effects of new production are about 3 times larger for all men than for all women ($47,000 versus $15,000). Columns (2) through (4) show that this pattern of relatively larger earnings effects for men is true regardless of the educational attainment of these men and women.

Table 2.1 also reports the own-county effects of new fracking production on the change in annual earnings per capita using the measure of potential new fracking production from Section 2.3.2 as an instrument for actual new production. The point estimates are consistently larger when using potential new production relative to the OLS estimates. Feyrer et al. (2017) also find consistently larger effects when instrumenting for new production in the same way. The effects of new production are noisily estimated when instrumenting however, and very few estimates are statistically significant, despite their relatively larger size.

Figure 2.4 shows point estimates of all the γ_d from equation (2.1), along with their 90 percent confidence intervals. These estimates capture whether and to what extent new fracking production in surrounding areas affects earnings within a county, even if that county itself does not engage in fracking. The distance 0 in the figure corresponds to the own-county effects from Table 2.1 discussed previously. The remaining estimates represent the effect of new fracking production within doughnuts of varying distances from a county.

One million dollars of new fracking production within counties zero to 40 miles away (not including own-county production) is associated with an earnings increase of about $11,100 for all workers, $5,200 for high-school-educated workers, $3,200 for workers with some college, and $1,500 for college-educated workers. One million dollars of new fracking production within counties that are between 40 and 60 miles away is associated with an earnings increase of about $5,100 for all, $2,400 for high-school-educated, $1,300 for some-college-educated, and $1,800 for college-educated workers. Fracking production beyond 60 miles from a county does not affect earnings, regardless of a worker's educational attainment. In Figure 2.5, I break the sample down further by gender and educational attainment and find that like the own-county effects of new production, the spillover effects are relatively larger for men than for women. For both genders,

[14] The estimated effects by educational attainment in columns (2) through (4) do not add up to the effect for all individuals in column (1) because the educational attainment characteristic was not available for some individuals in the QWI. I do not report the earnings effects of fracking for these individuals, but they are included in the "all individuals" category. This is true for the employment effects reported in Table 2.2 as well.

the spillover effects do not extend beyond 60 miles.

2.4.2 Employment

The employment effects of new production follow a similar pattern to those of the earnings effects. The OLS estimates in Table 2.2 suggest that one million dollars of new fracked oil and gas production is associated with an employment increase of about 1.03 within a county. This estimate is slightly larger, but similar to the effect documented by Feyrer et al. (2017) (0.85 at the county level).

This employment increase of 1.03 is not divided evenly by gender or by educational attainment. Of the 1.03 increase, 0.44 is for workers with an education of high school or less, 0.28 is for workers with some college, and 0.09 is for college-educated workers. The gender difference is even more stark, with employment increasing by 0.79 for all men, and only 0.12 for all women. This difference is also six to seven times larger for men of all education levels relative to women of the same education level. Instrumenting for new production also results in consistently larger estimated employment effects. Like with earnings, the effects of new production are noisily estimated when instrumenting. Few estimates are significant, their relatively larger size notwithstanding.

Figure 2.6 shows point estimates of all the γ_d from equation (2.1), along with their 90 percent confidence intervals, when the outcome variable is the change in employment per capita. These estimates capture the extent to which new fracking production in surrounding areas affects employment within a county. One million dollars of new fracking production within counties zero to 40 miles away (not including own-county production) is associated with an employment increase of 0.11 for all workers, 0.047 for workers with an education of high school or less, 0.02 for workers with some college, and 0.01 for college-educated workers. I find similar but smaller employment effects resulting from new production in counties that are 40 to 60 miles away. Again, production beyond 60 miles from the centroid of a county does not appear to affect employment within a county. Figure 2.7 illustrates that the spillover effects on employment are relatively larger for men than women at all education levels, but don't extend beyond 60 miles away for either gender.

2.4.3 College Enrollment

There is a general consensus in the literature that labor market opportunities are greatly enhanced in areas that engage in fracking (Weber, 2014; Bartik et al., 2019; Feyrer et al., 2017; Krupnick and

Echarte, 2017; Maniloff and Mastromonaco, 2017), particularly for non-college-educated men (Cascio and Narayan, 2017; Kearney and Wilson, 2018; Neilson, 2020). The earnings and employment effects of own-county production that I document are consistent with these findings. My findings further suggest that fracking production up to 60 miles away may impact the labor market opportunities within a county, especially for non-college-educated men.

By increasing the opportunity cost of additional years of schooling, the increased earnings of less-educated individuals would lead us to predict that fracking activity would result in a reduction in college attainment within these counties. Table 2.3 contains point estimates of the effect of own-county new fracking production on college enrollment per capita by gender and level of institution. I find that overall, one million dollars of new fracking production is associated with a reduction of 0.04 enrollments. The majority of this enrollment reduction is coming from a decrease in enrollments at two-year institutions (-0.027, p-value 0.105). Instrumenting for new production using potential new production again yields much larger standard errors. In the case of enrollment however, no estimate is significantly different from zero, nor is there any consistency in how the estimates from instrumenting differ from the OLS results.

The labor market effects of new production I document previously suggest that the costs and relative returns to additional years of schooling are being affected by fracking production in nearby areas, even if a county itself does not engage in fracking. Moreover, it is not uncommon for individuals to leave their county of residence to attend a nearby college, especially four-year and above institutions. Figure 2.8 shows the results of regressing the one-year change in enrollments per capita on the value of new fracking production within doughnuts of various radii surrounding each county that has an institution of higher education. One million dollars of new fracking production in counties within 40 miles (not including own-county production, if any) is associated with a reduction of 0.02 enrollments at all institutions (p-value 0.134). Interestingly, while the own-county effects of new production are driven by changes in enrollments at two-year institutions, the spillover effects from new production in surrounding areas are driven by reductions in enrollment at four-year institutions. Of the 0.021 enrollments lost at all institutions, 0.016 of those are from four-year institutions (p-value 0.036).

Figure 2.9 separates the sample further by gender and level of institution and reveals that the reduction in enrollments at two-year institutions from own-county production are split fairly evenly by men and women. The spillover effects of nearby fracking at four-year institutions however, are largely driven by men (-0.011 versus -0.0053). I find little evidence that new fracking production beyond 40 miles has an effect on enrollment for either gender at any type of institution.

2.4.4 Discussion

As a whole, my results suggest that new fracking production has a significant effect on earnings, employment, and college enrollment. Unsurprisingly, new fracking production affects these labor market and educational outcomes the most within the counties where the fracking production is taking place. Controlling for own-county production, I also find that earnings, employment, and enrollment are affected by the extraction of fracked oil and gas up to 40 to 60 miles away. The spillover effects are about 1/6 of the magnitude of the own-county estimates when new production is zero to 40 miles away, and about 1/13 of the magnitude when it is 40 to 60 miles away. Relative to the own-county effects of new fracking production on employment, the spillover effects are about 1/9 the size when new production is zero to 40 miles away, and about 1/18 the size when it is 40 to 60 miles away. The spillover effect of newly fracked oil and gas within zero to 40 miles on enrollment however, is almost 1/2 that of the own-county effect. The relative size of the spillover effects on enrollment are consistent with people responding to changing incentives in counties surrounding new production, as well as people within fracking counties who, absent a fracking boom, would have travelled outside of their county to attend college.

I assess the economic significance of my results by considering how much new fracked oil and gas production occurred in aggregate over the sample, and by considering the amount of new production at different percentiles of the distribution of new production. Figure 2.10 shows the total value of fracked oil and gas production by year from 2006 to 2016. Between 2006 and 2016, about $355 billion worth of newly fracked oil and gas was produced in the United States, with the value of production peaking at just over $62 billion in 2014. Considering the enormous amount of new oil and gas being produced over the sample illustrates that not only are the point estimates statistically significant, their magnitude is economically meaningful as well.

Evaluating what this means for the typical fracking county is complicated by the extremely skewed distribution of production in counties where fracking is taking place. There were 718 counties between 2006 and 2016 that had some amount of new fracking production. The top ten percent of producing counties accounted for over 78 percent of the $355 billion worth of total new production over the sample, and the top five percent accounted for 62 percent. The top one percent alone accounted for almost 26 percent, while the bottom 50 percent of producing counties accounted for less than one percent. The above estimated effects of new fracking production are thus heavily concentrated in the relatively few top producing counties, and my results are most relevant for them and surrounding counties.

2.5 Conclusion

This analysis is among the few comprehensive examinations of the effects of fracking-induced labor demand shocks on earnings, employment, and college enrollment. Fracking is of particular interest because I observe the exact location of the wells being drilled. This allows me to estimate the effects of new fracking production occurring at varying distances from a county.

I find substantial increases in earnings and employment as a result of fracking. Each million dollars of new production within a county is associated with $68,000 of new earnings and about one new job within that county. The same amount of new production within zero to 40 miles (not including own-county production, if any) generates an earnings and employment increase that is about 1/9 to 1/6 the magnitude of the own-county effects. The effects of new production 40 to 60 miles away are about 1/18 to 1/13 the magnitude of the own-county effects. The own-county and spillover effects of new production on earnings and employment are the largest in magnitude for non-college-educated men.

By increasing the earning potential of less-educated individuals, fracking increased the opportunity cost of additional years of schooling. I find that one million dollars of new production within a county is associated with a reduction of 0.04 enrollments within that county. The same amount of non-own-county new production within zero to 40 miles generates a decline in enrollment that is almost 1/2 the size of the magnitude of the own-county effect. The own-county effects of new fracking production are largest in magnitude at two-year institutions, while the spillover effects are largest in magnitude at four-year institutions. I find no evidence that fracking production beyond 60 miles affects earnings, employment, or college enrollment.

For policy evaluation and impact analysis, it is important to understand the spatial dispersion of economic shocks. Over $355 billion worth of fracked oil and gas was produced between 2006 and 2016 in the United States, the vast majority of which coming from the top ten percent of oil and gas producing counties. I find sizable spillover effects of fracking production that, together with the own-county effects, have important implications for local labor markets and educational outcomes, particularly in and around high oil and gas producing counties.

Table 2.1: Own-County Earnings Effects of New Fracking Production by Educational Attainment

	Dependent Variable: One Year Change in Earnings Per Capita			
	All Individuals (1)	High School or Less (2)	Some College (3)	College Graduates (4)
***Panel A.* All Individuals**				
$NewProd_{i,t}$	$68,416^{***}$	$31,275^{***}$	$19,629^{***}$	$8,520^{***}$
	(23,148)	(10,493)	(6,862)	(2,082)
$\widehat{NewProd}_{i,t}$	$83,639$	$31,444$	$24,090$	$46,004^{**}$
	(73,699)	(33,018)	(21,089)	(19,865)
***Panel B.* Men**				
$NewProd_{i,t}$	$46,623^{***}$	$22,218^{***}$	$13,726^{***}$	$4,891^{***}$
	(16,013)	(7,677)	(4,293)	(661)
$\widehat{NewProd}_{i,t}$	$49,448$	$20,890$	$12,823$	$23,846^{**}$
	(47,890)	(23,824)	(12,641)	(9,675)
***Panel C.* Women**				
$NewProd_{i,t}$	$14,855^{***}$	$8,891^{***}$	$5,190^{***}$	$2,323^{***}$
	(2,743)	(3,109)	(859)	(490)
$\widehat{NewProd}_{i,t}$	$40,298$	$10,294$	$16,172$	$21,786^{*}$
	(29,610)	(9,516)	(13,459)	(11,669)

Notes: This table reports point estimates from regressing the one-year change in annual earnings per capita by educational attainment on the value of own-county new production, measured in million dollars per capita. I control for new production in neighboring counties that are in donuts of various radii from the centroid of the county. I also control for year and county fixed effects, and a single lag of new production per capita within the county and in neighboring counties. Also reported are point estimates using *potential* new production as an instrument for new production (for details, see section 2.3.2). The sample includes all counties that have higher education institutions. The number of county-year observations in each regression is 11,988. Standard errors, clustered at the county level, are in parenthesis. Data sources: Enverus and the 2006-2016 Quarterly Workforce Indicators.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 2.2: Own-County Employment Effects of New Fracking Production by Educational Attainment

	Dependent Variable: One Year Change in Employment Per Capita			
	All Individuals (1)	High School or Less (2)	Some College (3)	College Graduates (4)
***Panel A.* All Individuals**				
$NewProd_{i,t}$	1.028***	0.439***	0.276***	0.090***
	(0.302)	(0.131)	(0.076)	(0.023)
$\widehat{NewProd}_{i,t}$	1.474	0.610	0.457*	0.513*
	(1.047)	(0.454)	(0.267)	(0.269)
***Panel B.* Men**				
$NewProd_{i,t}$	0.792***	0.310***	0.208***	0.106***
	(0.226)	(0.107)	(0.054)	(0.026)
$\widehat{NewProd}_{i,t}$	1.099	0.509	0.291*	0.072
	(0.674)	(0.324)	(0.154)	(0.099)
***Panel C.* Women**				
$NewProd_{i,t}$	0.122***	0.042***	0.036***	0.018***
	(0.029)	(0.009)	(0.021)	(0.005)
$\widehat{NewProd}_{i,t}$	0.923	0.346	0.406	0.220
	(0.748)	(0.285)	(0.275)	(0.158)

Notes: This table reports point estimates from regressing the one-year change in employment per capita by educational attainment on the value of own-county new production, measured in million dollars per capita. I control for new production in neighboring counties that are in donuts of various radii from the centroid of the county. I also control for year and county fixed effects, and a single lag of new production per capita within the county and in neighboring counties. Also reported are point estimates using *potential* new production as an instrument for new production (for details, see section 2.3.2). The sample includes all counties that have higher education institutions. The number of county-year observations in each regression is 11,988. Standard errors, clustered at the county level, are in parenthesis. Data sources: Enverus and the 2006-2016 Quarterly Workforce Indicators.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 2.3: Own-County Enrollment Effects of New Fracking Production by Institution Level

	Dependent Variable: One Year Change in Enrollment Per Capita			
	All Institutions	Four Year	Two Year	Less Than Two Year
	(1)	(2)	(3)	(4)
***Panel A.* All Individuals**				
$NewProd_{i,t}$	-0.042^{*}	-0.014	-0.027	-0.002
	(0.024)	(0.013)	(0.016)	(0.003)
$\widehat{NewProd}_{i,t}$	0.012	-0.030	0.022	0.021
	(0.338)	(0.107)	(0.333)	(0.028)
***Panel B.* Men**				
$NewProd_{i,t}$	-0.022^{*}	-0.007	-0.014^{*}	-0.001
	(0.013)	(0.008)	(0.008)	(0.003)
$\widehat{NewProd}_{i,t}$	0.120	0.017	0.086	0.017
	(0.253)	(0.040)	(0.252)	(0.016)
***Panel C.* Women**				
$NewProd_{i,t}$	-0.020	-0.007	-0.013	-0.001
	(0.013)	(0.006)	(0.010)	(0.003)
$\widehat{NewProd}_{i,t}$	-0.108	-0.047	-0.064	0.004
	(0.189)	(0.085)	(0.142)	(0.016)

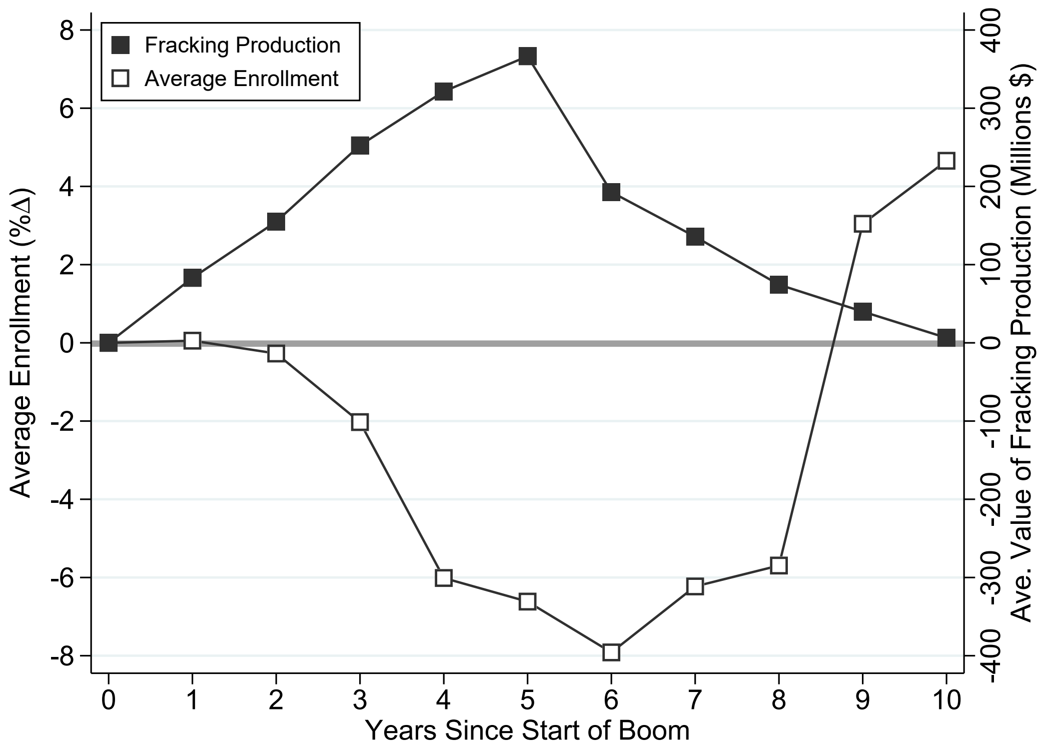
Notes: This table reports point estimates from regressing the one-year change in fall enrollment per capita by institution level on the value of own-county new production, measured in million dollars per capita. I control for new production in neighboring counties that are in donuts of various radii from the centroid of the county. I also control for year and county fixed effects, and a single lag of new production per capita within the county and in neighboring counties. Also reported are point estimates using *potential* new production as an instrument for new production (for details, see section 2.3.2). The sample includes all counties that have higher education institutions. The number of county-year observations in each regression is 11,998. Standard errors, clustered at the county level, are in parenthesis. Data sources: Enverus and the 2006-2016 Quarterly Workforce Indicators.

* Significant at the 10% level.

** Significant at the 5% level.

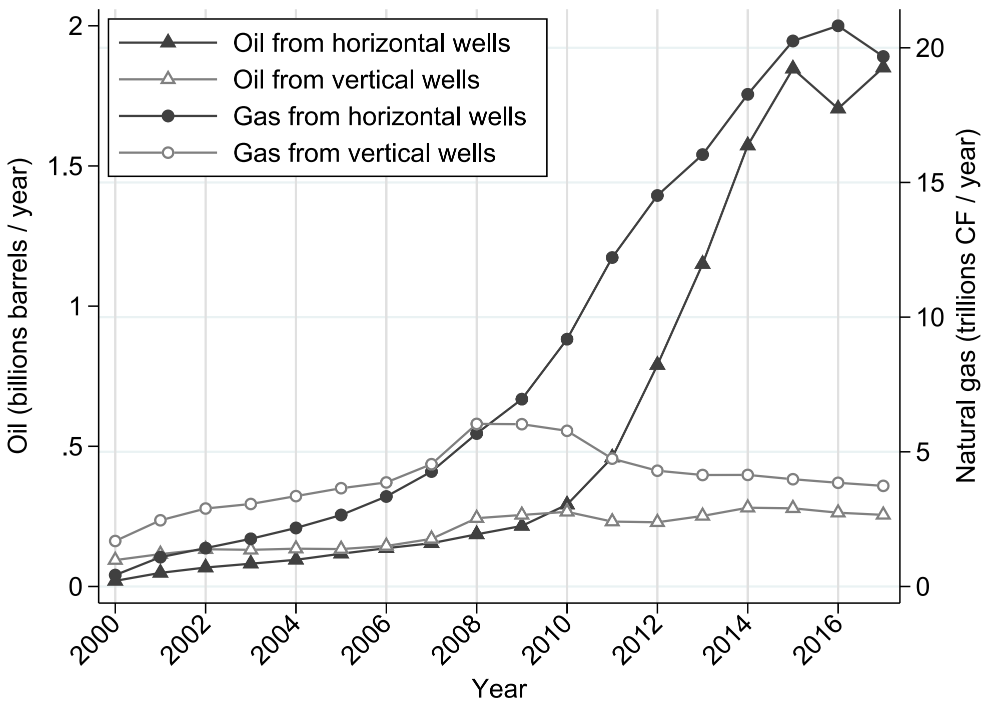
*** Significant at the 1% level.

Figure 2.1: College Enrollment and Fracking Production in Boom Counties



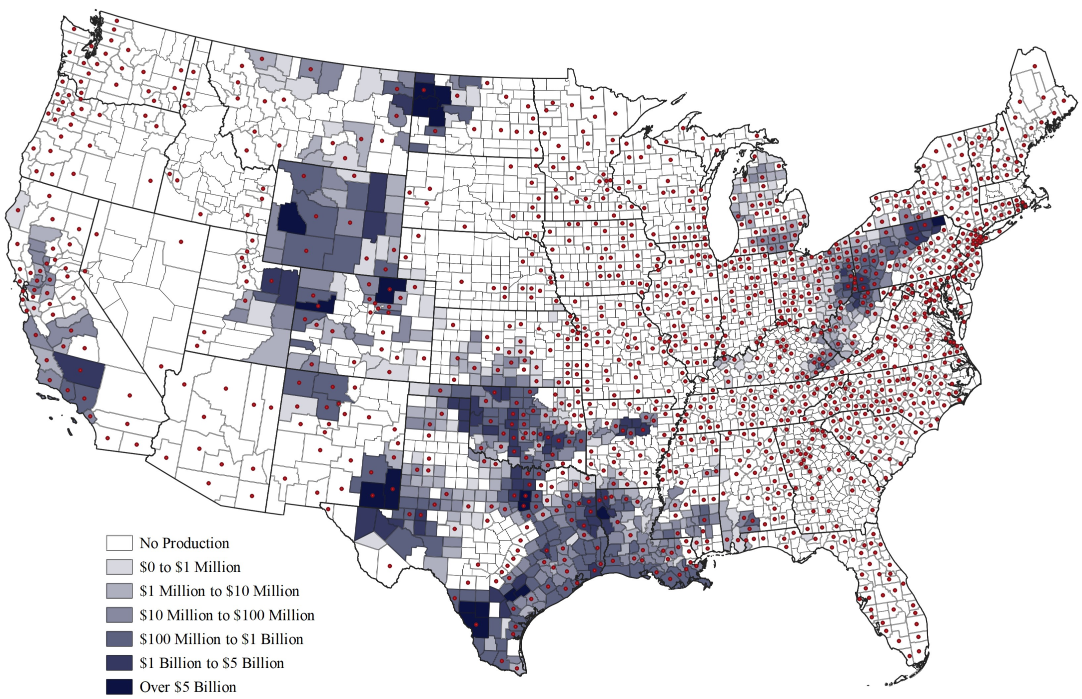
Notes: This figure contains the percentage change in average enrollments at four-year institutions within fracking boom counties, as well as the value of fracking production in these counties. Both measures are expressed relative to their respective values in the year in which the boom started in each county. The identification of boom counties and the year in which the boom started in each county come from Neilson (2020). Data sources: Enverus and the 2006-2016 Integrated Post Secondary Education Data System.

Figure 2.2: U.S. Production by Drill Type



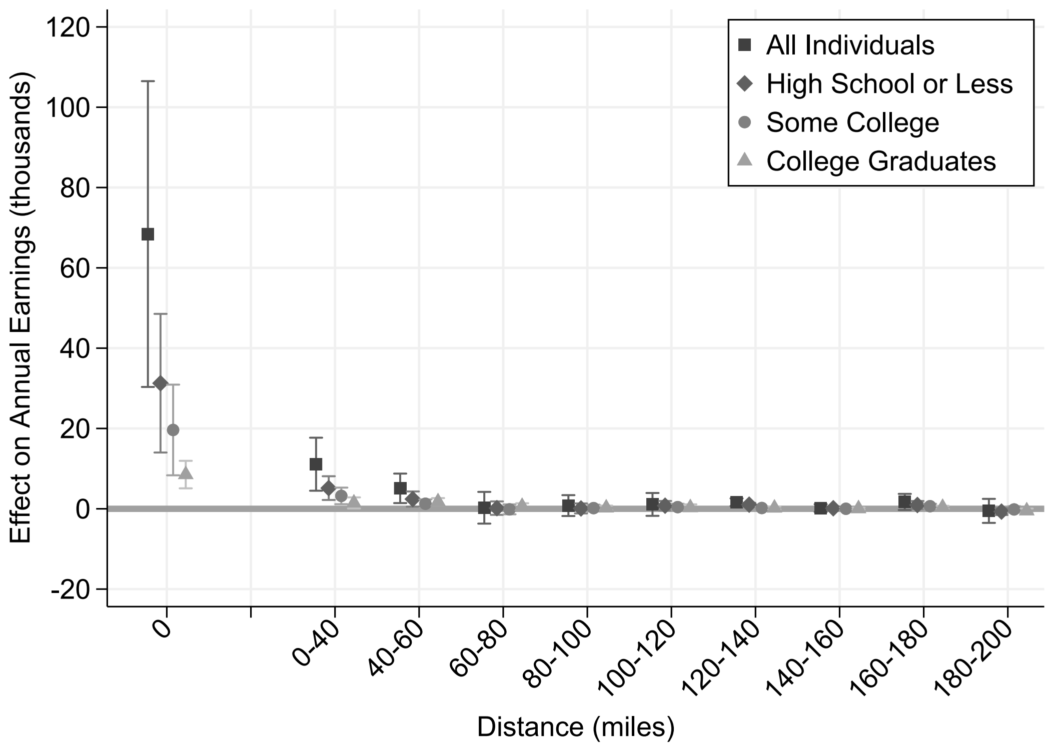
Notes: This figure contains yearly aggregates of oil and gas production with a drilling type of vertical or horizontal (including directional). I consider all non-vertically drilled wells as fracked wells. These aggregates come from wells with first production date in the year 2000 or later. Data source: Enverus.

Figure 2.3: U.S. Institutions of Higher Education and Cumulative New Fracking Production by County (2007 - 2016)



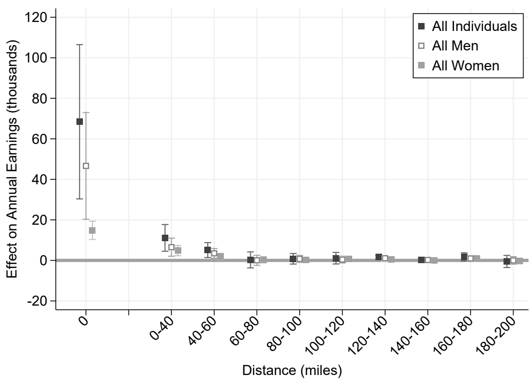
Notes: The red dots indicate counties that have at least one institution of higher education. The blue represents the cumulative value of fracked oil and gas production in these counties between 2007 and 2016. Data sources: Enverus and the Integrated Post Secondary Education Data System.

Figure 2.4: Earnings Effects by Educational Attainment Including Doughnuts of Neighboring Counties

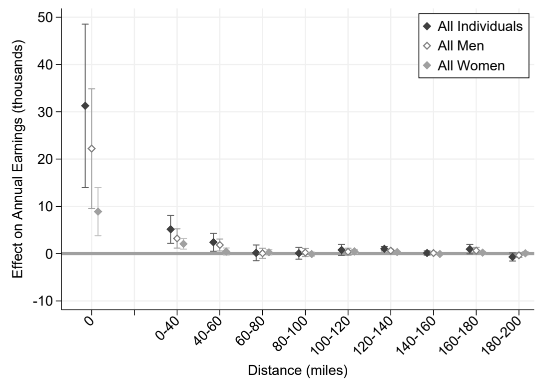


Notes: This figure contains point estimates and 90 percent confidence intervals from regressing the one year change in annual earnings per capita by educational attainment in a county on the value of new production per capita in doughnuts within a certain radius from the centroid of that county. I control for year and county fixed effects, and a single lag of new production per capita within each doughnut. The sample consists of all counties that have higher education institutions. Standard errors are clustered at the county level. Data sources: Enverus and the 2006-2016 Quarterly Workforce Indicators.

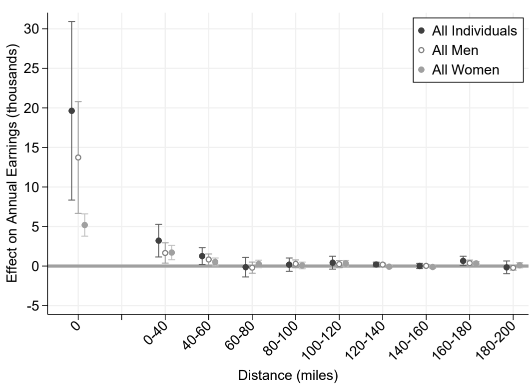
Figure 2.5: Earnings Effects by Educational Attainment and Gender Including Doughnuts of Neighboring Counties



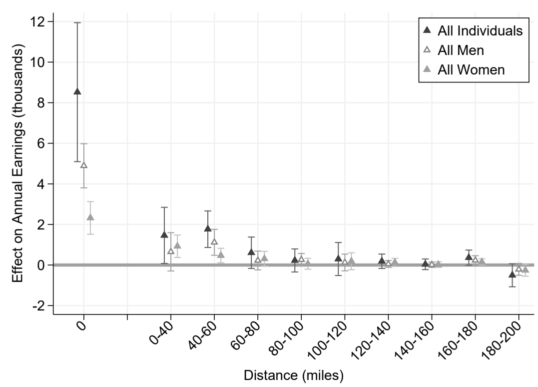
(a) All Individuals



(b) High School or Less



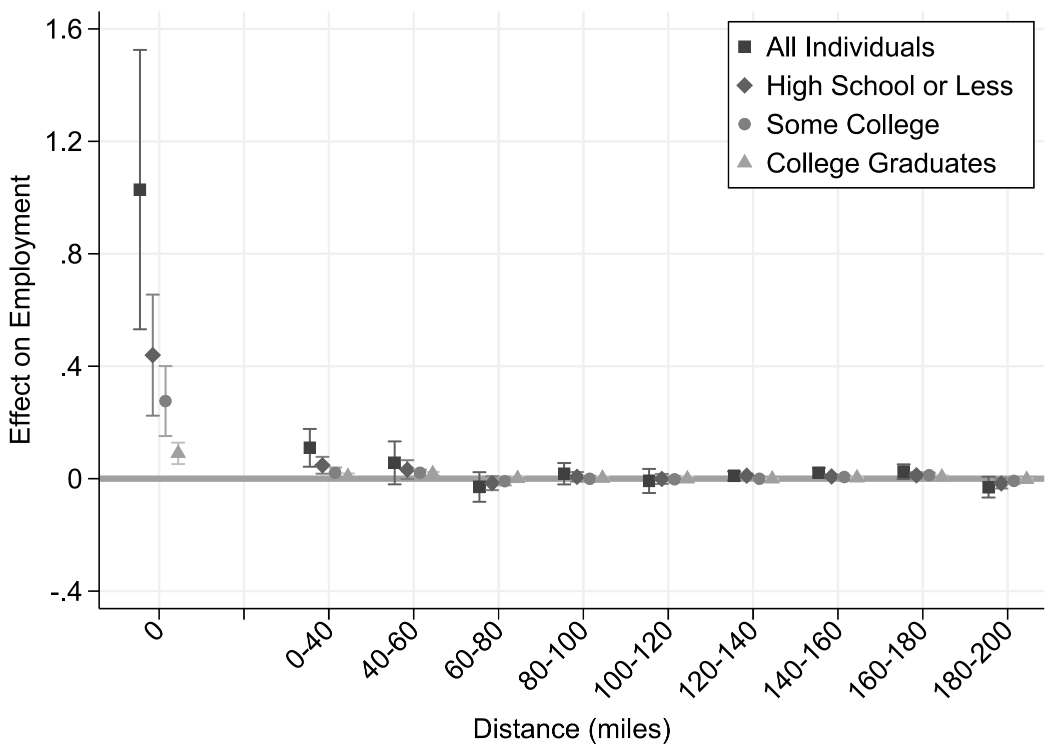
(c) Some College



(d) College Graduates

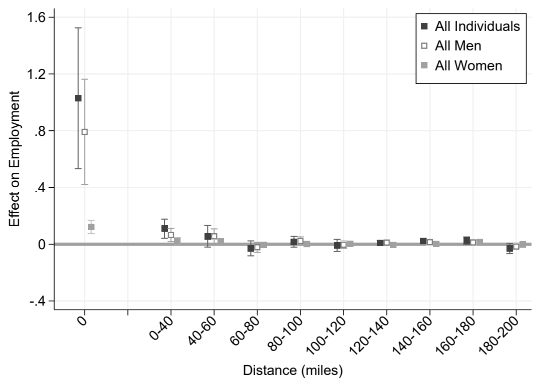
Notes: These figures contain point estimates and 90 percent confidence intervals from regressing the one year change in annual earnings per capita by educational attainment and gender in a county on the value of new production per capita in doughnuts within a certain radius from the centroid of that county. I control for year and county fixed effects, and a single lag of new production per capita within each doughnut. The sample consists of all counties that have higher education institutions. Standard errors are clustered at the county level. Data sources: Enverus and the 2006-2016 Quarterly Workforce Indicators.

Figure 2.6: Employment Effects by Educational Attainment Including Doughnuts of Neighboring Counties

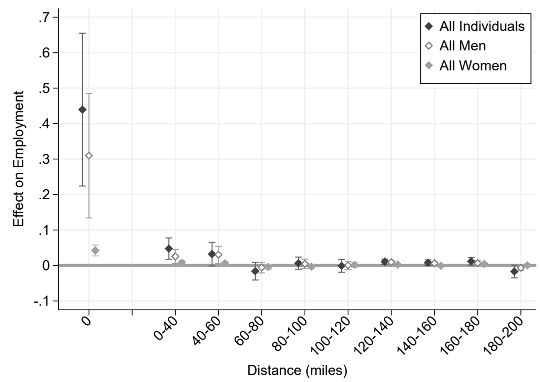


Notes: This figure contains point estimates and 90 percent confidence intervals from regressing the one year change in annual employment per capita by educational attainment in a county on the value of new production per capita in doughnuts within a certain radius from the centroid of that county. I control for year and county fixed effects, and a single lag of new production per capita within each doughnut. The sample consists of all counties that have higher education institutions. Standard errors are clustered at the county level. Data sources: Enverus and the 2006-2016 Quarterly Workforce Indicators.

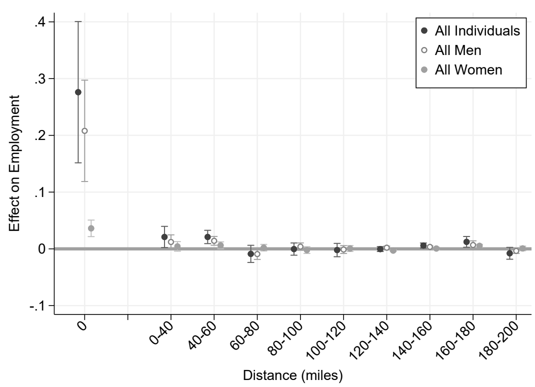
Figure 2.7: Employment Effects by Educational Attainment and Gender Including Doughnuts of Neighboring Counties



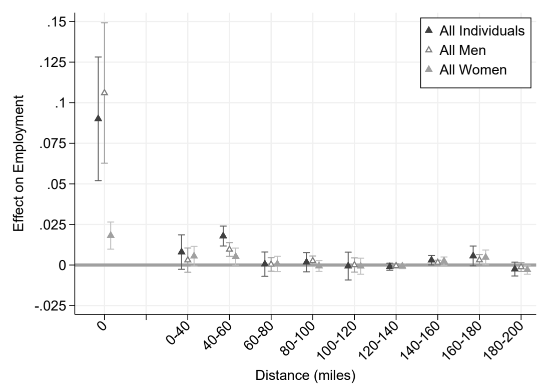
(a) All Individuals



(b) High School or Less



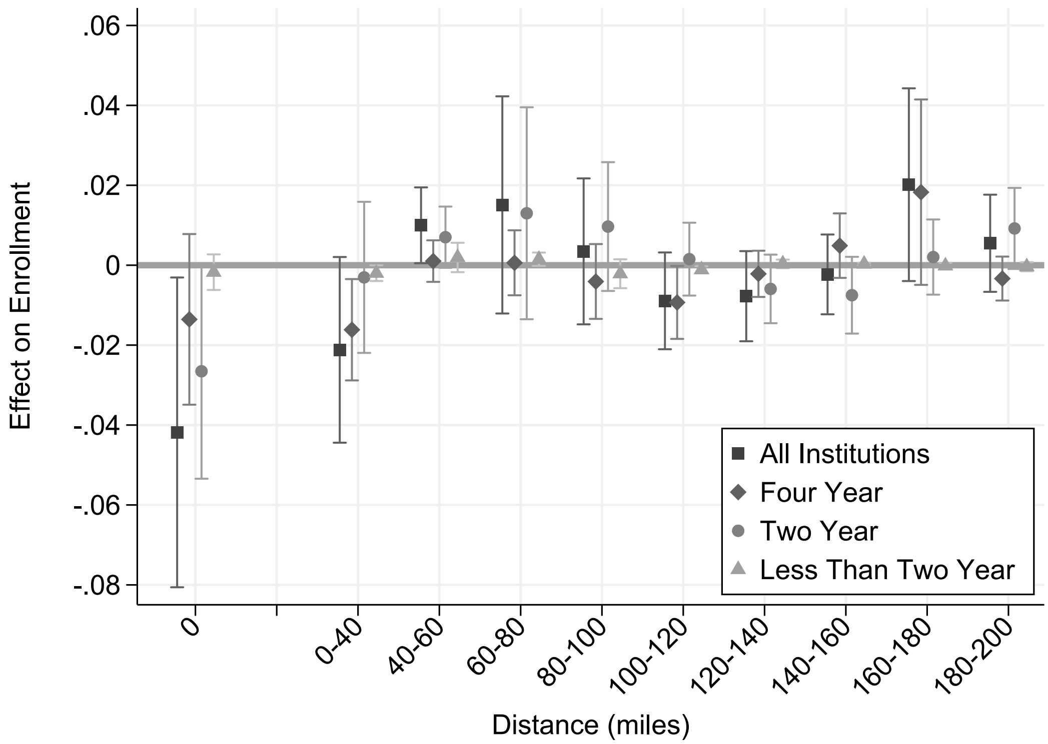
(c) Some College



(d) College Graduates

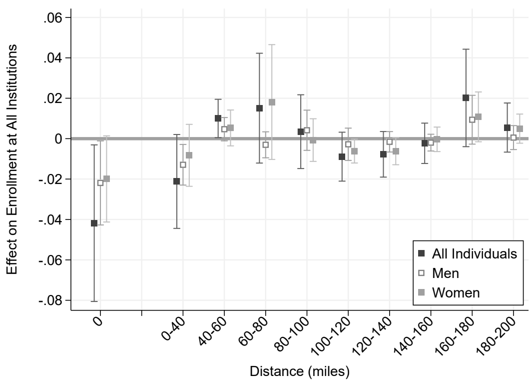
Notes: These figures contain point estimates and 90 percent confidence intervals from regressing the one year change in annual employment per capita by educational attainment and gender in a county on the value of new production per capita in doughnuts within a certain radius from the centroid of that county. I control for year and county fixed effects, and a single lag of new production per capita within each doughnut. The sample consists of all counties that have higher education institutions. Standard errors are clustered at the county level. Data sources: Enverus and the 2006-2016 Quarterly Workforce Indicators.

Figure 2.8: Enrollment Effects by Institution Level Including Doughnuts of Neighboring Counties

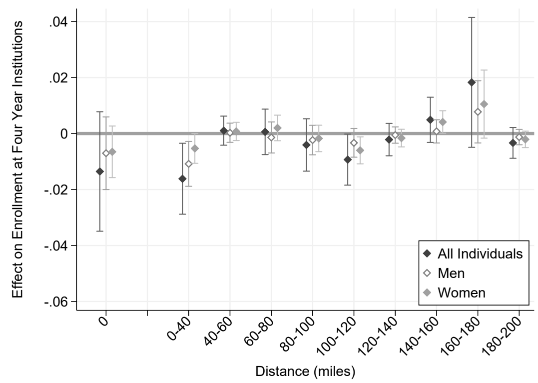


Notes: This figure contains point estimates and 90 percent confidence intervals from regressing the one year change in enrollment per capita by institution level in a county on the value of new production per capita in doughnuts within a certain radius from the centroid of that county. I control for year and county fixed effects, and a single lag of new production per capita within each doughnut. The sample consists of all counties that have higher education institutions. Standard errors are clustered at the county level. Data sources: Enverus and the 2006-2016 Integrated Post Secondary Data System.

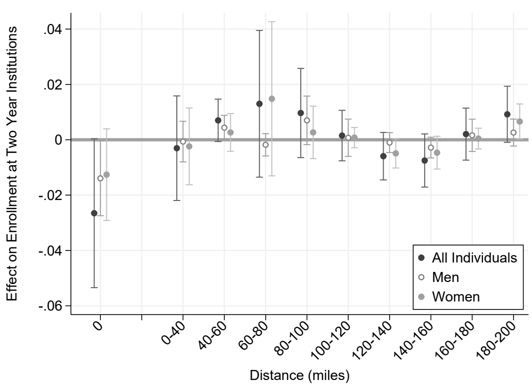
Figure 2.9: Enrollment Effects by Institution Level and Gender Including Doughnuts of Neighboring Counties



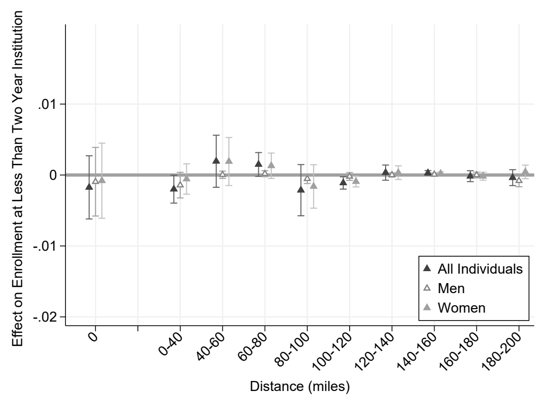
(a) All Institutions



(b) Four Year Institutions



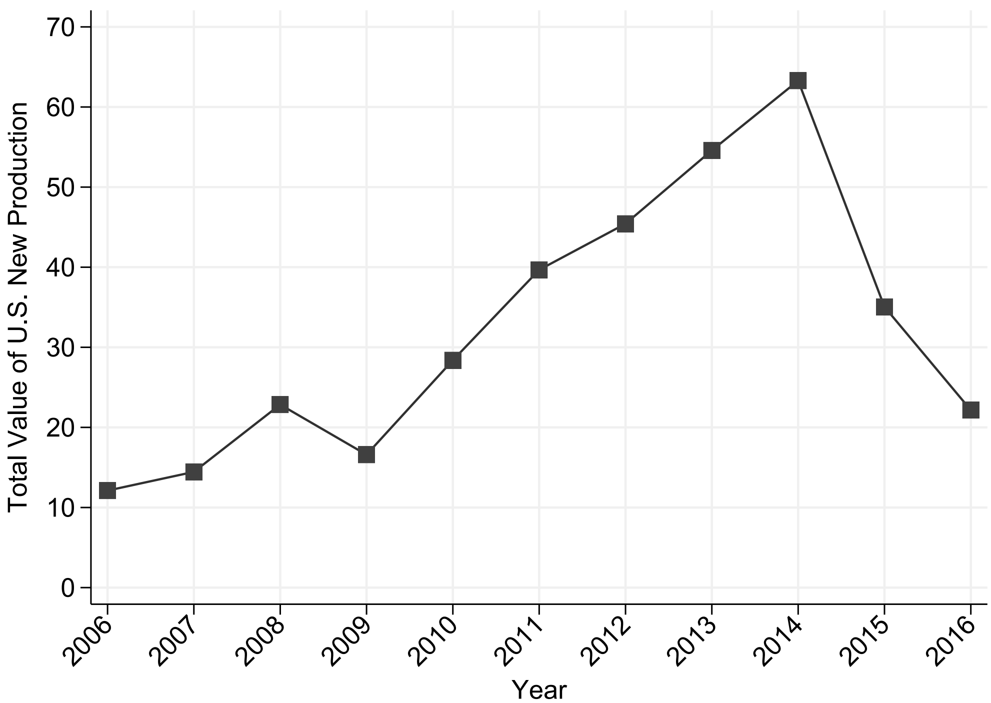
(c) Two Year Institutions



(d) Less than Two Year Institutions

Notes: These figures contain point estimates and 90 percent confidence intervals from regressing the one year change in enrollment per capita by institution level and gender in a county on the value of new production per capita in doughnuts within a certain radius from the centroid of that county. I control for year and county fixed effects, and a single lag of new production per capita within each doughnut. The sample consists of all counties that have higher education institutions. Standard errors are clustered at the county level. Data sources: Enverus and the 2006-2016 Integrated Post Secondary Data System.

Figure 2.10: Total Value of U.S. Fracked Oil and Gas by Year (2006 - 2016)



Notes: This figure contains the total value of new fracked oil and gas production in the United States between 2006 and 2016. These aggregates come from wells with first production date in the year 2000 or later. Data source: Enverus.

Chapter 3

The Effect of Unilateral Divorce Laws on College Educational Attainment

3.1 Introduction

In a stylized model of human capital investment, a decrease in the probability of future labor force participation unambiguously predicts a decreased motive to invest in higher education, *ceteris paribus*. Motivated by the fact that unilateral divorce laws in states with equal division of assets resulted in decreased female employment, as shown in Voena (2015), we test whether this distortion of the labor supply decision endogenously affected the human capital investment decisions of women.[1] The presence of unilateral divorce may induce other behavioral responses in the marriage market, which in turn could positively or negatively distort the human capital decisions of not just women, but men also. For example, making divorce easier to obtain, i.e. "cheaper" in price theoretic terms, unilateral divorce could influence who selects into marriage (Rasul, 2005).[2] Moreover, the selection into marriage, itself, affects the size of the marital surplus that is generated by the marital match.

Exploiting the temporal and spatial variation in the adoption of unilateral divorce laws by U.S. states, we find evidence of an effect of unilateral divorce laws on female human capital in-

[1]This chapter was co-authored by Peter Blair, an Assistant Professor at Harvard University in the Graduate School of Education, a Faculty Research Fellow of the National Bureau of Economic Research, and a Research Affiliate of the Human Capital and Economic Opportunity Network. Email: peter_blair@gse.harvard.edu.

[2]It also changes the identity of the marginal married couple that selects into divorce.

vestment that is consistent with the predictions of a stylized model. Women in states that adopted unilateral divorce laws are less likely to report obtaining a bachelor's degree or higher when compared to women in states with mutual consent divorce laws. We also find that men in states with unilateral divorce laws are less likely to report obtaining a bachelor's degree or higher than men in states with mutual consent divorce laws.

We find that the effect of unilateral divorce on the human capital investment decision is mediated by the nature of the property division laws in the state, which determine how marital assets are divided after divorce. There are three broad categories of property division laws under divorce: title-based division laws, community property division laws, and equitable division laws (Voena, 2015). These property division laws vary in the extent to which they align the private incentives of an individual spouse to invest in human capital with those of the couple. Under title-based property division laws, property at divorce is awarded based on individual ownership; therefore, each spouse fully internalizes the effect of his/her human capital investment decision on his/her ability to accumulate assets that are not subject to sharing upon divorce. Because of this feature, we take title-based division laws as our baseline for comparison. Community property division laws exist on the other end of the spectrum. Under these community property division laws, assets are split 50/50 at the dissolution of the marriage. Under this regime, in marriages where one spouse specializes in home-production and the other spouse specializes in working outside of the home, neither spouse fully internalizes the benefits of acquiring human capital as an individual, potentially leading to agency problems that cause under-investment in human capital in the presence of unilateral divorce. Equitable distribution laws, in which courts divide assets based on a notion of equity, which can mean that the division reflects the spouse with the greatest need or the spouse who made the greater contribution to creating the marital surplus, exist between the extremes of title-based property division laws and community property division laws.

In the data we find no effect of unilateral divorce laws on human capital investment decisions of individuals under equitable distribution, regardless of race and gender. However, we find negative and significant effects of community property laws on the human capital decisions of both women and men, with the effects being strongest and significant for whites. Since equitable distribution laws do not distort the human capital decisions of individuals, regardless of race, we can rule out race as the direct cause of the under-investment of whites in college degrees, when faced with community property division laws. Instead, the racial difference in the response to community property division laws in states with unilateral divorce laws could be related to racial differences in wealth, which alter the relative stakes of divorce. Upwards of 85.4% of marriages are same-race marriages (Passel et al., 2010), and the median wealth of white households is 18 times that of black households (Taylor et al., 2011); therefore we cannot rule out the possibility that differences in wealth result in difference in the marital surplus in which the absolute financial stakes of divorce are larger for white couples than black couples – hence the larger distortionary effect on the human capital decisions of white men and women.

Children are an important non-financial "asset" of many marriages. Not only is it common for married couples to have children, during the time of the divorce revolution it was not uncommon for one parent to specialize in taking care of the children and the home while the other parent worked.[3] To complement our study of property division laws as mechanisms through which unilateral divorce laws influence human capital investment, we investigate the extent to which unilateral divorce laws affected human capital investment through its interaction with gender-neutral child custody laws. Gender-neutral laws provided courts greater autonomy to grant custody to married fathers. In our analysis, we take non-gender-neutral laws as our baseline, since prior to the divorce revolution, mothers mostly retained custody of the children. We find that human capital investment does not depend significantly on the child custody laws in place, irrespective of whether a state has unilateral or mutual consent divorce laws.

Our work contributes to the literature on the impact of unilateral divorce laws in two ways. First, we document the potential for these laws to distort the human capital decisions of women and men. Johnson and Mazingo (2000) and Bronson (2014) are two other papers that study the impact of unilateral divorce laws on the decision to invest in human capital. The focus of Johnson and Mazingo (2000) is on understanding how unilateral divorce laws affected future outcomes of individuals who were exposed to unilateral divorce as children. Consistent with our findings, they conclude that women with many years of childhood exposure to unilateral divorce laws have completed less schooling. Contrary to what we find however, they conclude that unilateral divorce laws have no significant effect on the human capital investment decision of men who were exposed the laws in childhood. A key difference in our approach is that Johnson and Mazingo (2000) use a single cross-section of the Census 10 years after the divorce revolution, whereas we use two census

[3]For example, in 1970 about half of all married couples had children (Vespa et al., 2013), and only 4% of wives out-earned their husbands (Taylor et al., 2010; Schwartz and Gonalons-Pons, 2016).

years, one before and the other after the divorce revolution, in addition to 30 years of CPS data in order to exploit the time-series variation in divorce law adoption.

Bronson (2014) studies the effect of unilateral divorce laws on the human capital decisions of individuals using time series variation from a repeated cross-section. In our study, we focus on the effect of unilateral divorce laws on the absolute level of human capital investments for women and men, whereas Bronson focuses on the effect of these laws on the gender gap in human capital investment. In order for unilateral divorce laws to affect the gender human capital gap, it must affect the level of human capital investment for either women, or men, or both women and men. We find that unilateral divorce laws reduce the human capital investment of both men and women, but it reduces the human capital attainment of men by slightly more, which results in the convergence in the gender human capital attainment gap that Bronson (2014) documents.

Our work further contributes to the literature by showing that the impact of unilateral divorce laws on educational attainment vary systematically by race and depend crucially on property division laws upon divorce. This is complementary to the work of Bronson (2014) and Johnson and Mazingo (2000) where the focus is on gender and where unilateral divorce laws are not treated differently depending on the property division laws. Our results show that unilateral divorce laws have a significant negative effect on the human capital decisions of whites, especially in states with community property laws, but no significant effects on blacks, even in states with community property laws. The overall sign of these effects is the same across gender, holding race constant, but different across race, holding gender to be the same.

Moreover, we advance the literature by accounting for differences in the property division laws in unilateral divorce states and the gender neutrality of custody laws as potential mechanisms that explain differences in human capital. Voena (2015) finds that female employment depends crucially on whether a state has unilateral divorce laws and community property division or some alternative property division regime. In our study, we also find that this distinction is important for understanding the human capital investment decision, with the presence of community property laws leading to under-investment in human capital for whites. We build on Voena (2015) by: (i) adding an analysis of the effect of unilateral divorce laws on men, which Bronson (2014) captures by using gender differences as an outcome of interest; and (ii) by allowing for the effect of unilateral divorce laws to vary by race.

3.2 Brief Literature Review

Over the past 50 years, legislation governing the marital contract in the United States has changed in important ways. One of the most significant law changes affecting marriages occurred during the 1970s and 1980s, when many U.S. states adopted unilateral divorce laws allowing either spouse to initiate divorce proceedings unilaterally. Prior to a state adopting a unilateral divorce law, obtaining a divorce in that state would have required either the mutual consent of both spouses or fault grounds for the divorce.

The direct effect of the "divorce revolution" on divorce rates is well-studied. There is a growing consensus among economists that divorce rates rose sharply following the adoption of unilateral divorce laws, but that this rise was reversed within one decade (Peters, 1986; Allen, 1992; Peters, 1992; Friedberg, 1998; Wolfers, 2006). In addition to its effect on divorce rates, researchers have also studied the effect of divorce laws on female labor force participation (Peters, 1986; Gray, 1998; Stevenson, 2008; Voena, 2015), investment in marriage-specific capital (Stevenson, 2007), marriage rates (Rasul, 2005), children's welfare (Gruber, 2004), domestic violence (Stevenson and Wolfers, 2006), and crime (Cáceres-Delpiano and Giolito, 2012). In this paper we use data from the Current Population Survey (CPS) and the US Census to study the effect of unilateral divorce laws on the human capital investment decisions of individuals.

3.3 Data and Empirical Strategy

3.3.1 Coding Divorce Reform Dates

There are three main classifications for the laws which govern the division of property upon divorce, as outlined in this direct quote from Voena (2015):

1. *Title-Based Regimes* in which marital assets are divided according to the title of ownership;

2. *Community Property Regimes*, in which marital assets, presumed to be jointly owned, are divided equally between the spouses;

3. *Equitable Distribution Regimes*, in which courts have discretion in allocating marital assets in order to achieve equity. This may result in equal division or in a division that either favors the spouse who contributed most to obtain the asset or the spouse in most financial need.

In the early 1900s, the dominant legal regime for property division upon divorce was based on formal title of ownership. During this time, the eight states which had community property division laws were the only exceptions. Over time, states began to shift away from division based on property title towards equitable distribution division laws. During the 1970's many states also began to change not only laws affecting property division upon divorce, but also the process for initiating a divorce. This shift took the form of replacing divorce laws that required mutual spousal consent for divorce with unilateral divorce laws, which allowed for either spouse to initiate divorce proceedings. In addition to changes in divorce and property division laws, many states during the 1970s shifted from non-gender-neutral to gender-neutral child custody laws. Under the traditional tender years doctrine, custody almost always went to the mother in the event of divorce.[4] In the midst of the divorce revolution however, laws recognizing fathers' rights were introduced that provided courts freedom to grant custody to married fathers.

In Table 3.1, we report the timing of the adopting of unilateral divorce laws using the coding of Friedberg (1998) and Wolfers (2006), the timing of the shifts in the property division laws from title-based regimes to equitable distribution regimes using the coding of Voena (2015), and the timing of the shifts from non-gender-neutral to gender-neutral child custody laws using the coding of Rose and Wong (2014).

3.3.2 Data

For our empirical specification, we rely on both the US Census from 1970 and 1980 and the annual March Current Population Survey (CPS) from 1967 to 1999. Both data sources contain information on educational attainment and demographic characteristics of a nationally representative sample of individuals. The advantage of the Census data is its size. Although we only have two census years, we have a sample size of over 2 million observations, which increases the power of our study relative to the CPS, which has 30 years of data, but 1/3 as many observations over the entire sample period. The Census also has clear state identifiers for each of the sample years, whereas the CPS in some years has non-unique state identifiers for states, as noted in Gray (1998) and Stevenson (2008).[5] Although we use all available state-years, this issue further reduces the power of the CPS

[4] For a detailed historical account of the development and eventual abolition of the tender years doctrine, see Mason (1996) and Rose and Wong (2014).

[5] The states affected between 1968 and 1972 include: Alaska, Alabama, Arkansas, Arizona, Colorado, Delaware, Hawaii, Iowa, Idaho, Kansas, Massachusetts, Maine, Michigan, Minnesota, Mississippi, Montana, North Carolina, North Dakota, Nebraska, New Hampshire, New Mexico, Nevada, Oklahoma, Rhode Island, South Carolina, South Dakota, Utah, Virginia,

sample. Although the CPS is under-powered relative to the Census, the advantage of the CPS is that we observe individuals each year. The higher frequency of observation in the CPS, relative to the Census, allows us to better exploit the timing variation in the adoption of unilateral divorce laws, property division regimes, and gender-neutral child custody laws to estimate their effect on the human capital decision of individuals (Table 3.1).

In our analysis, we will use the CPS data to establish the sign of the effect of unilateral divorce laws on human capital, and in cases where the CPS may be under-powered, we will look to the estimates from the Census data to provide us more precise estimates. Ideally, we would like to have administrative IRS data with racial identifiers for implementing our approach, or even a public ACS-like data set from the 1970s, which would give us a large sample size and a high sampling frequency. We do not have both a large sample size and high frequency in one data set which is why we rely on this hybrid approach using the Census and CPS data.

We restrict our sample to individuals between the ages of 18 and 35 because schooling and marriage decisions are most likely to occur during these years. Similar to Goldin et al. (2006), we define our primary dependent variable "$grad_{ist}$" equal to one if respondent i in state s reports having completed at least 4 years of college or at least a bachelor's degree in year t. In Table 3.2, we provide descriptive statistics for the Census data and in Table 3.3 we provide descriptive statistics for the CPS. Both samples look similar in terms of the average age of respondents, an average of 26 years old, the number of children 1.05, the number of children under 5 - 0.43 versus 0.45 - and also the racial composition of the sample 85% white. As expected, the average wages are higher in the CPS, as is the mean college attainment. There is a simple answer for these differences, the CPS sample covers later years going into 1999, whereas the Census sample covers just 1970 and 1980, which are the two census years right before the "divorce revolution" and right afterwards.[6]

3.3.3 Empirical Specifications

For our empirical analysis, we follow the literature in using a differences-in-differences approach that exploits the temporal and spatial variation in the adoption of unilateral divorce laws, equitable

Vermont, Washington, Wisconsin, and Wyoming. The states affected between 1973 and 1976 include: Alaska, Alabama, Arkansas, Arizona, Colorado, Delaware, Georgia, Hawaii, Iowa, Idaho, Kansas, Kentucky, Louisiana, Maine, Maryland, Michigan, Minnesota, Mississippi, Missouri, Montana, North Dakota, Nebraska, New Hampshire, New Mexico, Nevada, Oklahoma, Oregon, Rhode Island, South Carolina, South Dakota, Tennessee, Utah, Virginia, Vermont, West Virginia, Washington, Wisconsin, and Wyoming.

[6]It is natural that both wages and educational attainment are higher on average over a longer sample period covering later years.

division property laws, and gender-neutral custody laws across states to estimate the causal effect of these regimes on human capital attainment of individuals. This empirical strategy is used in several papers in the literature to examine the causal impact of unilateral divorce laws on a variety of outcomes (Cáceres-Delpiano and Giolito, 2012; Friedberg, 1998; Gray, 1998; Gruber, 2004; Stevenson, 2007, 2008; Stevenson and Wolfers, 2006; Voena, 2015; Wolfers, 2006). The plausible exogeneity of the introduction of unilateral divorce laws has been established by this rich literature and the plausible exogeneity of the shift to equitable distribution division laws has been demonstrated in Voena (2015), who shows the timing of the introduction of these laws in 27 states is uncorrelated with the female employment rate in 1960 and the wives' share of household income in 1960.

Our first specification is the following linear probability model:

$$grad_{ist} = \beta U_{st} + \gamma X_{ist} + \psi Z_{st} + \alpha_s + \delta_t + \varepsilon_{ist}, \tag{3.1}$$

where the *dependent* variable "$grad_{ist}$" equals one if respondent i in state s reports having completed at least four years of college or at least a bachelor's degree in year t. The *independent* variable of interest U_{st} is a dummy variable that takes a value of one for a state s that had already adopted unilateral divorce by year t. The coefficient of interest β captures the effect of introducing a unilateral divorce regime on the probability of graduating college relative to a mutual consent regime. The vector of individual demographic controls, X_{ist}, includes: age, marital status, total number of children, number of children under the age of 5, and race;[7] Z_{st} is a vector of state-level demographic and policy controls including age composition variables indicating the share of states populations aged 26 to 40, 41 to 55, 56 to 65, and over 65, the share of the state's population that is black, the natural log of state personal income per-capita, and the state's law regarding abortion access (Donohue III and Levitt, 2001); and α_s and δ_t represent state and year fixed effects, respectively.

3.3.3.1 Property Division Laws, Child Custody Laws, & Human Capital Investment

Our second empirical model allows for the human capital investment decision to depend on both the existence of unilateral divorce laws and their interaction with the property division regime in the state. Intuitively, the property rights division laws determine how the marital surplus will be

[7] Insofar as marital status and number of children are affected by unilateral divorce directly, including them as controls could be problematic. Our results change only slightly when we omit these variables as controls, and the estimated sign and statistical significance of our estimates remain unchanged.

divided upon divorce whereas the existence of unilateral divorce laws capture how easy it is to access this division of the marital surplus. Changes along both of these margins can influence the returns to investing in human capital for both men and women in a marital relationship.

Given traditional gender roles, in which one spouse may specialize in home-production and the other in paid labor, the property division laws are particularly important for determining the extent to which each spouse fully internalizes the costs and benefits of investing in human capital. Under a *title-based* division law, each person exits the marriage with the assets that he or she owns outright, therefore there is full internalization of the costs and benefits of human capital investment. For this reason, our benchmark for comparison will be the level of human capital investment in a policy environment in which there is mutual consent laws and title-based division of assets.

Community property regimes exist on the other end of the spectrum. Since, under these laws, assets are split 50/50, irrespective of title or other considerations. Under this regime, the cost and benefits of the individual human capital decision are jointly borne by both spouses, which can create a misalignment of incentives that distorts the human capital investment decision relative to our benchmark of full individual internalization of the cost and benefits of investing in human capital under title-based division.

Somewhere between these two extremes is the *equitable distribution* property division regime. Under this framework, the court has discretion to award assets in a way that promotes equity. This could result in an allocation that favors the spouse with the greatest financial need or one that favors the spouse who made the most significant contribution to the marital surplus. When compared to our benchmark of full internalization, this regime is a type of second-best solution in that it takes into account an imperfect measure of effort, or need/sacrifice, whereas the title-based division allows for full internalization of the investment choice.[8]

We follow Voena (2015) in estimating the following triple difference model, in which we exploit the variation in both unilateral divorce laws and the type of property division regime to estimate their heterogeneous effects on human capital investment decisions. We add to the literature by estimating the following equation with a new outcome of interest – the decision to complete a

[8]One could also argue that the equitable distribution can create more agency problems than the community property regime because it introduces uncertainty with respect to the division of assets, i.e. the division rests on what an ex-ante unknown judge deems equitable. In practice, strong precedent may remove some of this ambiguity and, on average, provide a reasonably sharp prior on what is considered to be an equitable division both according to the law and in the eyes of the "reasonable observer."

college degree – and by estimating this equation on sub-samples of men, as well as by breaking out our analysis by race (given gender and marital status):

$$
\begin{aligned}
grad_{ist} = {} & \beta_1(U_{st} \times CommProp_{st}) + \beta_2(U_{st} \times Title_{st}) + \beta_3(U_{st} \times EquitDistr_{st}) \\
& + \beta_4 CommProp_{st} + \beta_5 EquitDistr_{st} + \gamma X_{ist} + \psi Z_{st} + \alpha_s + \delta_t + \varepsilon_{ist}. \quad (3.2)
\end{aligned}
$$

In our estimating equation, $CommProp_{st}$, $Title_{st}$, and $EquitDistr_{st}$ are dummy variables taking on the value of one if state s has a community property, title-based, or equitable distribution regime in year t (respectively), and zero otherwise. The coefficients $\beta_1, \beta_2, \beta_3$ capture, respectively, the effect of introducing unilateral divorce relative to mutual consent divorce on the college degree attainment of individuals in states with community property, title-based and equitable distribution regimes. The coefficients β_4 and β_5 capture the effect of having community property and equitable distribution regimes on college attainment relative to title-based laws in states with mutual consent divorce.

Our third empirical model is similar to the second, but it allows for the human capital investment decision to depend on both the existence of unilateral divorce laws and their interaction with the child custody law in the state:

$$
\begin{aligned}
grad_{ist} = {} & \beta_6(U_{st} \times GendNeut_{st}) + \beta_7(U_{st} \times NonGendNeut_{st}) \\
& + \beta_8 GendNeut_{st} + \gamma X_{ist} + \psi Z_{st} + \alpha_s + \delta_t + \varepsilon_{ist}. \quad (3.3)
\end{aligned}
$$

$GendNeut_{st}$ and $NonGendNeut_{st}$ are dummy variables taking on the value of one if state s has gender-neutral or non-gender-neutral custody laws in year t (respectively), and zero otherwise. The coefficients β_6 and β_7 capture, respectively, the effect of unilateral divorce relative to mutual consent divorce on college degree attainment of individuals in states with gender-neutral and non-gender-neutral custody laws. The coefficient β_8 captures the effect on college attainment of having gender-neutral custody laws relative to non-gender-neutral custody laws in states with mutual consent divorce laws.

3.3.3.2 Synthetic Control Method

We use the above differences-in-differences estimations to get at the overall effects of unilateral divorce laws on college attainment. To examine the potential heterogeneity in treatment effects by state, we use the synthetic control approach introduced by Abadie and Gardeazabal (2003) and Abadie et al. (2010). The idea behind the synthetic control approach is that observed quantifiable characteristics can be used to identify a combination of untreated units that provide an appropriate comparison for the treated unit or units. In the context of this study, a synthetic control for each unilateral state is obtained as a weighted-average of mutual consent divorce states. The weights are obtained in fitting the pre-treatment trends in college graduation in unilateral states with the synthetic of other mutual consent states based on pre-intervention state characteristics. For each state-year, the characteristics we use for this matching include: the proportion of individuals aged between 26 and 40, 41 and 55, 56 and 65, and over 65, the proportion of males, the proportion of black individuals, the natural log of real per-capita income, and as suggested by Abadie et al. (2010), the proportion of college graduates at the beginning, middle, and end of the pre-intervention period. In addition to the ability to check for heterogeneity in estimated effects by state, one advantage of the synthetic control method relative to our other specifications is that the effects of observables are not assumed to be time-invariant. Matching on pre-intervention characteristics and outcomes can implicitly match on unobservables over time.[9] Because of the lack of unique state identifiers for each year in the CPS between 1968 and 1976, one drawback to the synthetic control method for our analysis is that it requires a balanced data set.[10] We therefore perform the analysis on the eleven states that have unique identifiers in every year over the entire sample. Namely, available treated states include California, Connecticut, Florida, Indiana, and Texas; available untreated states include the District of Columbia, Illinois, New Jersey, New York, Ohio, and Pennsylvania.[11]

The effect of the passage of unilateral divorce laws can be interpreted for each post-intervention period, as the difference between the outcome in each unilateral state and its respective synthetic control. Since we look at four treated states with different treatment years between 1970 and 1974 in our sub-sample, for each post-treatment year we find an average period-specific treatment effect by averaging the period-specific effects over the treated states. To make our synthetic control re-

[9]For technical details on the synthetic control method as we have implemented it, see Abadie and Gardeazabal (2003), Abadie et al. (2010), and Cavallo et al. (2013).

[10]This data issue is not unique to our study, see for example Stevenson (2008) and Gray (1998).

[11]We were unable to find a synthetic control that matched Connecticut well based on pre-intervention characteristics, especially when analyzing our sub-sample of males. Therefore, we omit Connecticut from the analysis that follows.

sults comparable to our difference-in-difference results, we also report the difference in the mean differences, between college graduation rates in unilateral states and that of the synthetic control, in the pre-treatment period from that in the post-treatment period.

To test the significance of our estimated average effects, we follow Cavallo et al. (2013) in constructing a distribution of average placebo effects and assessing how our estimated effect ranks in that distribution. Specifically, for each of the four treatment years in our sub-sample, we perform the synthetic control method on each of the untreated states, the idea being that in the absence of unilateral divorce, a reduction in college graduation rates in the placebo state relative to its synthetic control is not expected. The probability that our estimated effect would occur by chance is simply the number of average placebo effects that are smaller (more negative) than our estimated average treatment effect, divided by the total number of possible placebo averages.[12]

3.4 Results

Figure 3.1 shows the aggregate proportion of female graduates in adopting and non-adopting states over time using annual data from the March CPS from 1967 to 1999. The dashed vertical red lines indicate the period over which the majority of adopting states adopted unilateral divorce (see Table 3.1). The two groups of states are almost identical up through the late 1970s, at which point they begin to diverge with the adopting states growing more slowly than the non-adopting states, indicating that women in adopting states are not graduating as much on average compared to women in non-adopting states. Figures 3.2 and 3.3 show the aggregate proportion of female graduates in adopting and non-adopting states over time by marital status and race. Even in these aggregate trends of the raw data we see stark racial differences in educational attainment in unilateral and non-unilateral states over time. The proportion of white female graduates in unilateral and non-unilateral states are similar up until the late 1970s, after which the proportion of female graduates grows more quickly in non-unilateral states than unilateral states. This is evident for both married and unmarried white females. No such divergence exists for black females, regardless of their marital status.

Figure 3.11 shows the aggregate proportion of male graduates in adopting and non-adopting states over time. Similar to females, up until the late 1970s, the two groups of states have a nearly

[12] Since there are six untreated states and four different treatment periods, we find a total of 1,296 different possible placebo average effects.

identical proportion of male graduates. After 1980, however, the proportion of male graduates in unilateral states remains stagnant around 18%, but in non-unilateral states the proportion continues to grow. Figures 3.12 and 3.13 show that this pattern is evident for white men, whether they are married or unmarried. There is no clear difference however, in the proportion of black male graduates in unilateral and non-unilateral states over time, even when taking into account their marital status.

3.4.1 Results for Women

In Table 3.4 we report the results from our differences-in-differences specification in equation (3.1), in which we do not account for differences in property division laws or child custody laws, using separately the data from the Census (columns 1-4) and the Data from the CPS (columns 5-8). Both the results from the US Census sample (Table 3.4, column 4) and the results from the CPS sample (Table 3.4, column 8) suggest that women in states with unilateral divorce laws are about 1 percentage point (or 7%) less likely to report graduating from college than women in states with mutual consent divorce laws.

In Table 3.5 we report the effect of unilateral divorce on the reported college attainment of black women and white women (separately) and also on married women and unmarried women (separately). We do this for both the US census (columns 1-4) and the CPS (columns 5-8). From this exercise we find negative effects of unilateral divorce on the reported college attainment of women in each of these 4 categories in both data sets. In particular, we find significant negative effects of unilateral divorce on white women in both data sets. White women are 1.2 percentage points (p.p.) less likely to report having a college degree in unilateral divorce states.[13] Moreover, this effect appears to be concentrated among married women, who in both samples are about 1.2 p.p. less likely to report having a college degree.[14] The estimated effect on unmarried women is smaller in magnitude and indistinguishable from zero in both samples, whereas the estimated effect of these laws on black women is only statistically significant in the Census sample and moreover one half the magnitude of the estimated effect for white women. In Table 3.6, we further disaggregate the married sub-sample of women by race and the unmarried sub-sample of women by race so that we have the effect of unilateral divorce laws on married white women, unmarried white women, mar-

[13] This effect is statistically significant at the 5% level in the Census sample and statistically significant at the 10% level in the CPS sample.

[14] In both the census and the CPS this effect is statistically significant at the 10% level).

ried black women and unmarried black women for both the Census samples and the CPS samples. Here we find significant negative impacts of unilateral divorce on the reported college attainment of married white women (-1.2 p.p.) and unmarried black women (-1.0 p.p.), using the Census sample. The point estimate for unmarried white women is similar in the CPS as in the Census, but less precisely estimated, whereas the point estimate for unmarried black women in the CPS is one third the size, even though the decrement in precision is not substantial. Taken together, the results from the Census and CPS suggest that the negative effect of unilateral divorce, which we observed for women in Table 3.4 is largely driven by married white women reporting lower levels of college attainment. As predicted by our simple model of human capital investment, a fall in human capital investment for these women is consistent with the result in Voena (2015), who found that married women are less likely to be in the labor market following the passage of unilateral divorce.

In Table 3.7, we report the effects of divorce laws and property rights division laws on female human capital investment from our empirical specification in equation (3.2). In the unrestricted sample of all women, we find a positive and significant effect of equitable division laws relative to title-based division in states with mutual consent divorce laws. These results suggest that equitable distribution laws in the presence of mutual consent divorce laws either induce positive selection into marriages or, on-balance, create an environment in which women are encouraged to invest in human capital.[15] The interaction between unilateral divorce laws and equitable distribution laws, however, is statistically insignificant, which suggests that the threat of unilateral divorce does not change the incentive for women to invest in education in states with equitable division laws.

By contrast, there is a negative but statistically insignificant effect of community property division on female human capital investment in mutual consent states, relative to title-based laws. The presence of unilateral divorce laws in states with equal division of property is associated with a decline of 1.41 p.p. in the probability that a woman reports obtaining a bachelor's degree or higher. This effect is comparable to the over-all decline in female human capital that we observed in our first specification, in which we did not break out the unilateral divorce laws based on the property division regime (see Table 3.4). Since community property division laws guarantee a 50/50 split of the marital surplus, in a traditional marital arrangement, this could create a disincentive for

[15] This inducement for women to invest in human capital could, for example, be due to the marriage relaxing credit constraints to paying for college given spousal income.

women to invest in human capital because the community property division laws act as a type of insurance against their spouse exiting the marriage. It could also be the case that in states with unilateral divorce laws and community property that there is negative selection of women into marriages – women with lower levels of education are more likely to get married.

When we break results out into sub-samples by race and marital status, a striking pattern emerges along racial lines. The benefits of equitable distribution laws in mutual consent states are strongest among married white women. Although the point estimates for unmarried white women and married black women are statistically insignificant – we notice that positive benefits of equitable distribution laws in the presence of mutual consent are larger for married women than single women regardless of race, which is consistent with a story in which positive selection into marriage in states with equitable distribution laws is the dominant mechanism.[16]

The negative effects on reported college attainment which we observed for community property division laws in the full female sample appear to be concentrated among white women. In states with unilateral divorce, married white women reduce human capital investment by 1.47 p.p. and unmarried white women by 2.44 p.p. (both statistically significant - Table 3.7, columns 6 and 7). This suggests a dual role for both negative selection of white women into marriage in these states and under-investment in human capital. The corresponding reductions for black women are 3-10 times smaller and moreover statistically insignificant (Table 3.7, columns 8 and 9). By contrast, we observe a large positive statistically significant effect of community property laws in mutual consent states for black women. This effect is twice as large for married black women (5.5 p.p.) relative to single black women (2.4 p.p.). The advent of unilateral divorce laws does not significantly alter the positive benefits of community property laws on black female human capital investment.

In Figure 3.4 we summarize the large racial differences in human capital investment between white women and black women in states with community property laws. These differences are accentuated by the presence of unilateral divorce laws, for both married and unmarried women. Most of the differences are being driven by white women reducing human capital investment in the presence of unilateral divorce laws. One potential explanation, is that given that most marriages are same-race marriages, and moreover given that there are large racial differences in wealth, the

[16] As reported in Figure 3.5, there are no racial differences in the effect of equitable distribution laws on female human capital investment, regardless of whether the state has a mutual consent or unilateral divorce regime.

size of the marital surplus is much larger in white marriages than black marriages, hence the larger human capital distortion for white women.

In Table 3.8, we report the effects of divorce laws and child custody laws on female human capital investment from our empirical specification in equation (3.3). In states with gender-neutral child custody laws, we find no significant difference in educational attainment of women living unilateral divorce compared to those living under mutual consent divorce laws, regardless of marital status or race. Similarly, we find null results in states with non-gender-neutral custody laws. Furthermore, in unilateral states and mutual consent states, we find no significant difference in educational attainment of females living under gender-neutral or non-gender-neutral custody laws. Even with the relative ease of obtaining a divorce under the unilateral regime, human capital investment appears to be unaffected by changing from non-gender-neutral to gender-neutral custody laws.

As an alternative to our differences-in-differences research design, we study the effect of unilateral divorce laws on women's reported educational attainment by using the synthetic control method described in section 3.3.3.2. Figure 3.6 contains trends in the average proportions of female college graduates over time in the states that adopted unilateral divorce laws and also in their synthetic controls. Figure 3.7 shows the effect of unilateral divorce based on the number of years since unilateral was introduced, i.e. the difference between the average proportions of graduates in unilateral states and their synthetic controls. Subtracting the mean difference in the proportion of female graduates between the unilateral states and their synthetic control in the post-treatment period from that in the pre-treatment period, we find that on average unilateral divorce caused about a 3.5 p.p. decrease in the likelihood of females graduating college.

In Figure 3.8, we plot the empirical distribution of the effects of unilateral divorce in our control states. This exercise is a way of providing a confidence interval on our estimate of the effect of unilateral divorce from the synthetic control method. There are 1,296 possible combinations of these placebo effects, as described in section 3.3.3.2, of these none are smaller (more negative) than the -3.5 p.p. treatment effect. This suggests that our synthetic control estimate yields not only a significantly negative effect of unilateral divorce on women's reported educational attainment, but an effect that is larger and more precisely estimated than our differences-in-differences estimates.

Using the synthetic control method, we can also look at the treatment effect for four individual states separately: California, Florida, Indiana, and Texas.[17] In Figure 3.9, we show the trends in the proportion of female graduates in each of the unilateral states separately with their respective synthetic controls. In Figure 3.10, we show the difference in proportions in each year between the two. In all four of these states we see a similar pattern in which reported female college attainment falls in the unilateral divorce states relative to their synthetic counterparts, and moreover this fall follows an increasingly negative trend over time. Consistent with our previous findings concerning property division laws, the two states with community property division laws, California and Texas, experience a relatively larger effect than Indiana and Florida, who do not have community property laws.

3.4.2 Results for Men

In Table 3.9, we report the effects of unilateral divorce laws on male human capital investment, independent of property division and child custody laws. The negative effect of unilateral divorce laws is stronger and statistically significant in the CPS sample (-1.59 p.p.) and smaller in magnitude and statistically insignificant in the Census sample (-.61 p.p.). In Table 3.10 we report the effect of unilateral divorce on the reported college attainment of black men and white men (separately) and also on married men and unmarried men (separately). We do this for both the US Census (columns 1-4) and the CPS (columns 5-8). From this exercise we find negative effects of unilateral divorce on the reported college attainment of men in each of these four categories in both data sets. In particular, we find significant negative effects of unilateral divorce on white men in both data sets (-0.69 p.p. in the Census and 1.51 in the CPS). Black men are 0.24-0.34 p.p. less likely to report having a college degree in unilateral divorce states but this is statistically indistinguishable from zero in both the Census and CPS data sets. Moreover, this effect appears to be concentrated among married men, who are 0.94 p.p. and 1.87 p.p. less likely to report having a college degree in the Census and CPS samples, respectively.[18] In Table 3.11, we further disaggregate the married sub-sample of men by race and the unmarried sub-sample of men by race so that we have the effect of unilateral divorce laws on married white men, unmarried white men, married black men and unmarried black men for both the Census samples and the CPS samples. Here we find significant

[17] The synthetic control method requires a balanced data set, therefore we can only analyze states that were uniquely identified throughout the entire sample period (see sections 3.3.3.2 and 3.3.2.)

[18] The estimated effect on unmarried men is smaller in magnitude in both samples and a precise zero in the census sample (-0.06 p.p.) but a statistically significant decrease of 1.1 p.p. in the CPS sample.

impacts of unilateral divorce on the reported college attainment of married white men (-0.8 p.p. in the Census and -1.72 p.p. in the CPS) and unmarried white men (-1.2 p.p. in the CPS). There are no significant impacts on black men regardless of marital status in either data set. Taken together, the results from the Census and CPS suggest that the negative effect of unilateral divorce, which we observed for men in Table 3.9 is largely driven by married white men reporting lower levels of college attainment.

In Table 3.12, we report the effects of divorce laws and property rights division laws on male human capital investment from our empirical specification in equation (3.2). In the unrestricted sample of all men in column 1, we find a positive and significant effect of being in a state with equitable distribution division laws in the presence of mutual consent divorce, relative to title-based division laws in states with mutual consent. There is a negative but statistically insignificant effect of community property division on male human capital investment in mutual consent states, relative to title-based laws. These results suggest that equitable distribution laws in the presence of mutual consent divorce laws either induce positive selection into marriages or, on-balance, create an environment in which men are encouraged to invest in human capital.

The interaction between unilateral divorce laws and equitable distribution laws, however, is statistically significant, and sufficiently negative to negate the relative human capital benefit of equitable distribution laws, relative to title-based regimes, for men. Similarly, the interaction between unilateral divorce laws and community property laws is negative and significant. The presence of unilateral divorce laws is associated with a decline of 1.8 p.p. in the probability of a man reporting a bachelor's degree or higher in a state with community property division. This effect of unilateral divorce laws in community property states on male human capital is comparable to the over-all decline in male human capital due to unilateral divorce that we observed in our first specification, in which we did not breakout the unilateral divorce laws based on the property division regime in the state (see table 3.9).

We observed a similar overall negative effect of unilateral divorce laws on human capital in states with community property laws for women to the one we observe here for men, which is consistent with our theoretical paradigm of unilateral divorce laws lowering the cost of exposing the marital surplus to the incentive misalignment issue of community property laws.

When we break results out into sub-samples by race and marital status, a striking pattern emerges along racial lines. Unlike the female sample, where we saw positive effects of equitable distribution laws in states with mutual consent divorce for both black and white women, in the male sample we observe positive significant effects for white men only, regardless of marital status, and negative effects for black men, although the effect is only significant for unmarried black men. In the full sample, we found no overall effect of community property laws on the human capital decisions of men in states with mutual consent but this too masked substantial heterogeneity in the data. We find negative statistically significant effects of these laws for unmarried white men (-1.5 p.p.) and for married black men (-3.7 p.p.) but no statistically significant effects for either married white men or unmarried black men.

Unilateral divorce laws, however, equalize the distortionary human capital response of married white men (-2.5 p.p.) and married black men (-3.2 p.p.) in community property regimes, as can be seen in Figure 3.14. This finding would be consistent with a story in which married black men and married white men face different exposure to partner-initiated divorce in mutual consent states. If the exposure is higher for black men, then one would expect a stronger response to the distortion due to the 50/50 sharing rule from black men relative to white men. An alternative story, which is consistent with our finding that community property laws in states with mutual consent induce married black women to increase the probability of college attainment by 5.5 p.p., is that black women are the major contributors to the marital surplus and that the under-investment in human capital that we see at baseline from married black men reflects their spouse's human capital as a type of insurance in the case of divorce. This could also explain why unilateral divorce laws do not change the human capital decisions of black men in states with community property.

In Table 3.13, we report the effects of divorce laws and custody laws on male human capital investment from our empirical specification in equation (3.3). The results for males mirror those for females, human capital investment on average was not significantly affected by changing from non-gender-neutral to gender-neutral custody laws, regardless of divorce law regime, marital status, and race.

We also analyze the effect of unilateral divorce on reported college attainment of men using the synthetic control method outlined in section 3.3.3.2. Figure 3.16 contains trends in the average proportions of male college graduates over time in the unilateral states and that of their synthetic controls, while Figure 3.17 shows the effect of unilateral divorce based on the number of years since a unilateral regime was introduced. Subtracting the mean difference in the proportion of male graduates between the unilateral states and their synthetic control in the post-treatment period from that in the pre-treatment period, we find that if unilateral divorce had not been introduced in these states, the proportion of male graduates would have been about 4.8 p.p. higher over the 25-year post-treatment period.

In Figure 3.18, we plot the empirical distribution of the effects of unilateral divorce in our control states for men. There are 1,296 possible combinations of these placebo effects, as described in section 3.3.3.2, of these none are smaller (more negative) than the -4.8 p.p. treatment effect. This suggests that our synthetic control estimate yields not only a significantly negative effect of unilateral divorce on men's reported educational attainment, but an effect that is larger and more precisely estimated than our differences-in-differences estimates.

Figure 3.19 shows the trends in the proportion of male graduates in each of the unilateral states separately with their respective synthetic controls, and Figure 3.20 contains the year-specific effects of unilateral divorce over time. In all four states, the pattern is consistent with our previous findings of a negative effect of unilateral divorce on male college attainment. Also similar to females, California and Texas, the two states with community property division laws, experience a relatively larger effect than Indiana and Florida, who do not have community property laws.

3.5 Conclusion

From a theoretical perspective, changes in divorce law and property division laws upon divorce affect both the private and social returns to human capital investment. As such, changes in these laws influence an individual's human capital investment decision. This general equilibrium consequence of changes in unilateral divorce law and property division laws has received limited attention in the literature, its importance, notwithstanding. Moreover, even studies of the direct effect of these laws on divorce rates have largely abstracted from heterogeneity in the behavioral responses by race, which we find matters crucially in this policy context.

While we do find differing behavioral responses between whites and blacks in response to the regime changes that occurred during the "divorce revolution," the behavioral differences are not due to the direct effect of race on the outcomes of interest, but rather appear to be due to disparities in wealth between whites and blacks and the ways in which these disparities affect both the size of the marital surplus to be split by the couple and also how these marriages form in the first place. Social forces such as the high degree of same race marriages, which compound racial wealth disparities, and mass incarceration, which results in more female heads of households in black families are consistent with our finding that making divorce unilateral introduces less of a human capital distortion for black households because the surplus to be shared is smaller and also more likely to be jointly created rather than primarily created by the male spouse, as is more often the case in white marriages.

Table 3.1: Timing of Divorce Law Reforms

State	Unilateral Divorce	Equitable Distribution	Gender Neutral Custody	State	Unilateral Divorce	Equitable Distribution	Gender Neutral Custody
Alabama	1971	1984	1981	Montana	1975	1976	No
Alaska	Pre-1967	Pre-1967	1977	Nebraska	1972	1972	1976
Arizona	1973	Community Property	1973	Nevada	1973	Community Property	1979
Arkansas	No	1977	1987	New Hampshire	1971	1977	1975
California	1970	Community Property	No	New Jersey	No	1974	No
Colorado	1971	1972	1983	New Mexico	1973	Community Property	1971
Connecticut	1973	1973	1970	New York	No	1980	No
Delaware	No	Pre-1967	No	North Carolina	No	1981	1977
District of Columbia	No	1977	1972	North Dakota	1971	Pre-1967	No
Florida	1971	1980	No	Ohio	No	1981	No
Georgia	1973	1984	1975	Oklahoma	Pre-1967	1975	1986
Hawaii	1973	Pre-1967	1976	Oregon	1973	1971	No
Idaho	1971	Community Property	No	Pennsylvania	No	1980	No
Illinois	No	1977	1975	Rhode Island	1976	1981	No
Indiana	1973	Pre-1967	1977	South Carolina	No	1985	1996
Iowa	1970	Pre-1967	No	South Dakota	1985	Pre-1967	1979
Kansas	1969	Pre-1967	1977	Tennessee	No	Pre-1967	1997
Kentucky	1972	1976	1974	Texas	1974	Community Property	1974
Louisiana	No	Community Property	1979	Utah	No	Pre-1967	No
Maine	1973	1972	1981	Vermont	No	Pre-1967	No
Maryland	No	1978	1978	Virginia	No	1982	1982
Massachusetts	1975	1974	No	Washington	1973	Community Property	1981
Michigan	1972	Pre-1967	1971	West Virginia	No	1985	No
Minnesota	1974	Pre-1967	No	Wisconsin	No	Community Property (1986)	1981
Mississippi	No	1989	No	Wyoming	1977	Pre-1967	1977
Missouri	No	1977	No				

Note: Data on the adoption of unilateral divorce come from Friedberg (1998) and Wolfers (2006). Data on property division regimes come from Voena (2015). Data on gender neutral custody laws come from Rose and Wong (2014).

Table 3.2: Descriptive Statistics by Gender, Race, and Marital Status - U.S. Census

Variable	Full Sample		White		Black		Married		Unmarried	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Females:										
grad†:	0.13	0.34	0.14	0.35	0.07	0.26	0.14	0.35	0.13	0.34
Age:	25.95	5.11	25.97	5.11	25.66	5.09	27.64	4.59	22.36	4.17
Wage:	4,236.40	5,283.94	4,261.39	5,258.63	4,032.26	5,320.52	3,882.15	5,194.74	4,342.23	5,078.44
Number of Children:	1.05	1.29	1.03	1.25	1.25	1.48	1.55	1.30	0.14	0.54
Number of Children Under 5:	0.43	0.69	0.43	0.69	0.46	0.73	0.65	0.77	0.08	0.32
Black:	0.13	0.33	0	0	1	0	0.08	0.27	0.18	0.38
White:	0.85	0.36	1	0	0	0	0.89	0.31	0.80	0.40
Males:										
grad†:	0.17	0.38	0.18	0.39	0.07	0.25	0.21	0.41	0.13	0.33
Age:	25.90	5.11	25.95	5.11	25.44	5.10	28.45	4.27	22.65	4.17
Wage:	9,340.86	8,713.20	9,775.65	8,839.25	6,303.26	6,962.11	12,515.74	9,193.14	5,633.74	6,374.91
Number of Children:	0.70	1.12	0.71	1.11	0.63	1.19	1.39	1.24	0.01	0.14
Number of Children Under 5:	0.34	0.65	0.35	0.65	0.27	0.61	0.68	0.78	0.01	0.09
Black:	0.11	0.32	0	0	1	0	0.08	0.28	0.14	0.34
White:	0.86	0.35	1	0	0	0	0.89	0.31	0.83	0.37
Female Observations:	2,183,498		1,846,283		275,075		1,234,033		725,735	
Male Observations:	2,129,115		1,831,136		239,626		1,048,439		941,998	

Note: Samples are from the 1970 and 1980 U.S. Census surveys and consist of individuals between the ages of 18 and 35.
† *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college.

Table 3.3: Descriptive Statistics by Gender, Race, and Marital Status - Current Population Survey

	Full Sample		White		Black		Married		Unmarried	
Variable	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Females:										
grad†:	0.16	0.36	0.16	0.37	0.09	0.28	0.17	0.38	0.15	0.36
Age:	26.48	5.16	26.50	5.16	26.20	5.18	28.24	4.50	23.09	4.54
Wage:	6,913.04	10,139.20	6,993.52	10,178.14	6,043.44	9,126.88	6,648.66	10,291.87	6,922.82	9,718.36
Number of Children:	1.05	1.27	1.02	1.24	1.28	1.46	1.52	1.29	0.24	0.71
Number of Children Under 5:	0.45	0.71	0.44	0.70	0.49	0.76	0.67	0.77	0.13	0.43
Black:	0.11	0.31	0	0	1	0	0.07	0.25	0.17	0.37
White:	0.85	0.36	1	0	0	0	0.90	0.30	0.79	0.41
Males:										
grad†:	0.18	0.38	0.18	0.39	0.09	0.28	0.21	0.41	0.14	0.35
Age:	26.47	5.17	26.51	5.16	25.96	5.27	29.02	4.14	23.40	4.57
Wage:	13,096.64	14,801.52	13,507.78	14,960.46	9,170.85	11,404.83	17,008.77	16,006.53	8,752.37	12,002.49
Number of Children:	0.70	1.11	0.71	1.10	0.63	1.15	1.39	1.22	0.03	0.24
Number of Children Under 5:	0.35	0.65	0.36	0.65	0.28	0.61	0.69	0.78	0.02	0.15
Black:	0.09	0.29	0	0	1	0	0.07	0.25	0.11	0.31
White:	0.87	0.33	1	0	0	0	0.90	0.30	0.84	0.36
Female Observations:	634,722		540,021		70,845		345,662		226,140	
Male Observations:	590,435		515,272		53,110		285,541		271,403	

Note: Samples are from the Current Population Survey 1967 - 1999 and consist of individuals between the ages of 18 and 35.
† *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college (or at least has a bachelor degree in the 1992 to 1999 surveys).

Table 3.4: Specification (3.1) Results - Full Samples (Females)

	Dependent variable: grad†							
	US Census				Current Population Survey			
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unilateral††	0.0128 (0.0076)	-0.0062 (0.0040)	-0.0107 (0.0045)	-0.0102 (0.0044)	0.0019 (0.0090)	-0.0104 (0.0076)	-0.0116 (0.0058)	-0.0119 (0.0061
State Fixed Effects		✓	✓	✓		✓	✓	✓
Year Fixed Effects		✓	✓	✓		✓	✓	✓
Individual Demographic Controls			✓	✓			✓	✓
State-level Demographic & Policy Controls				✓				✓
Sample Size	2,156,568	2,156,568	2,156,568	2,156,568	634,722	634,722	634,722	634,722

Note: Individual demographic controls include age, marital status, total number of children, number of children under the age of 5, and race. State-level demographic and policy controls include age composition variables indicating the share of states' populations aged 26 - 40, 41 - 55, 56 - 65, and over 65, the share of the state's population that is black, the natural log of state personal income per-capita, and the state's law regarding abortion access. Standard errors, in parenthesis, are clustered by state.

† *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college.

†† Unilateral is a binary variable equal to 1 if the state has already adopted unilateral divorce at year *t* (in the case of the US Census, this takes the value of 1 if the year is 1980 and the state adopted the law between 1970 and 1980).

Table 3.5: Specification (3.1) Results - Samples Restricted by Marital Status and Race (Females)

Variable	Married Females (1)	Unmarried Females (2)	Black Females (3)	White Females (4)	Married Females (5)	Unmarried Females (6)	Black Females (7)	White Females (8)
	Dependent variable: grad[†]							
	US Census				Current Population Survey			
Unilateral [††]	-0.0115	-0.0037	-0.0069	-0.0120	-0.0133	-0.0098	-0.0026	-0.0119
	(0.0044)	(0.0049)	(0.0034)	(0.0051)	(0.0078)	(0.0076)	(0.0072)	(0.0066
State Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓	✓	✓
State-level Demographic & Policy Controls	✓	✓	✓	✓	✓	✓	✓	✓
Sample Size	1,217,197	717,934	273,872	1,821,371	345,662	226,140	70,845	540,021

Note: Individual demographic controls include age, marital status (when the sample is not restricted by marital status), total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). State-level demographic and policy controls include age composition variables indicating the share of states' populations aged 26 - 40, 41 - 55, 56 - 65, and over 65, the share of the state's population that is black, the natural log of state personal income per-capita, and the state's law regarding abortion access. Standard errors, in parenthesis, are clustered by state.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college (or has at least a bachelor degree in the 1992 to 1999 Surveys).

[††] Unilateral is a binary variable equal to 1 if the state has already adopted unilateral divorce at year *t*.

Table 3.6: Specification (3.1) Results - Samples Further Restricted by Marital Status and Race (Females)

	Dependent variable: grad†							
	US Census				Current Population Survey			
Variable	White Married Females (1)	White Unmarried Females (2)	Black Married Females (3)	Black Unmarried Females (4)	White Married Females (5)	White Unmarried Females (6)	Black Married Females (7)	Black Unmarried Females (8)
Unilateral ††	-0.0116 (0.0046)	-0.0074 (0.0065)	-0.0022 (0.0050)	-0.0101 (0.0039)	-0.0120 (0.0082)	-0.0119 (0.0085)	-0.0004 (0.0128)	-0.0037 (0.0077)
State Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓	✓	✓
State-level Demographic & Policy Controls	✓	✓	✓	✓	✓	✓	✓	✓
Sample Size	1,081,553	569,992	100,180	127,131	310,366	179,328	22,798	37,323

Note: Individual demographic controls include age, marital status (when the sample is not restricted by marital status), total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). State-level demographic and policy controls include age composition variables indicating the share of states' populations aged 26 - 40, 41 - 55, 56 - 65, and over 65, the share of the state's population that is black, the natural log of state personal income per-capita, and the state's law regarding abortion access. Standard errors, in parenthesis, are clustered by state.

† *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college (or has at least a bachelor degree in the 1992 to 1999 Surveys).

†† Unilateral is a binary variable equal to 1 if the state has already adopted unilateral divorce at year t.

Table 3.7: Specification (3.2) Results (Females)

	Dependent variable: grad†								
Variable	Full Sample (1)	Married Females (2)	Unmarried Females (3)	Black Females (4)	White Females (5)	White Married Females (6)	White Unmarried Females (7)	Black Married Females (8)	Black Unmarried Females (9)
Unilateral×CommProp	-0.0141	-0.0152	-0.0222	0.0004	-0.0144	-0.0147	-0.0244	-0.0020	-0.0074
	(0.0051)	(0.0070)	(0.0076)	(0.0074)	(0.0067)	(0.0086)	(0.0084)	(0.0130)	(0.0074)
Unilateral×Title	0.0092	0.0072	0.0199	0.0082	0.0088	0.0071	0.0200	0.0138	0.0086
	(0.0065)	(0.0091)	(0.0089)	(0.0099)	(0.0083)	(0.0100)	(0.0107)	(0.0134)	(0.0117)
Unilateral×EquitDistr	-0.0033	0.0001	-0.0004	-0.0004	-0.0017	0.0024	-0.0014	0.0051	0.0022
	(0.0063)	(0.0073)	(0.0078)	(0.0089)	(0.0066)	(0.0071)	(0.0093)	(0.0159)	(0.0085)
Commprop	-0.0009	-0.0021	0.0045	0.0276	-0.0053	-0.0036	-0.0033	0.0551	0.0244
	(0.0041)	(0.0039)	(0.0052)	(0.0058)	(0.0046)	(0.0044)	(0.0058)	(0.0156)	(0.0051)
EquitDistr	0.0123	0.0151	0.0045	0.0117	0.0139	0.0153	0.0119	0.0170	0.0081
	(0.0054)	(0.0048)	(0.0075)	(0.0061)	(0.0060)	(0.0053)	(0.0093)	(0.0138)	(0.0045)
State Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
State-level Demographic & Policy Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sample Size	634,722	345,662	226,140	70,845	540,021	310,366	179,328	22,798	37,323

Note: Individual demographic controls include age, marital status (when the sample is not restricted by marital status), total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). State-level demographic and policy controls include age composition variables indicating the share of states' populations aged 26 - 40, 41 - 55, 56 - 65, and over 65, the share of the state's population that is black, the natural log of state personal income per-capita, and the state's law regarding abortion access. Standard errors, in parenthesis, are clustered by state.

† *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college (or has at least a bachelor degree in the 1992 to 1999 Surveys).

Table 3.8: Specification (3.3) Results (Females)

	Dependent variable: grad[†]								
Variable	Full Sample (1)	Married Females (2)	Unmarried Females (3)	Black Females (4)	White Females (5)	White Married Females (6)	White Unmarried Females (7)	Black Married Females (8)	Black Unmarried Females (9)
Unilateral×GendNeut	-0.0105	-0.0134	-0.0014	-0.0028	-0.0125	-0.0128	-0.0071	-0.0004	0.0034
	(0.0091)	(0.0101)	(0.0111)	(0.0134)	(0.0087)	(0.0090)	(0.0142)	(0.0289)	(0.0122)
Unilateral×NonGendNeut	-0.0113	-0.0139	-0.0087	-0.0029	-0.0129	-0.0134	-0.0113	-0.0042	-0.0046
	(0.0083)	(0.0104)	(0.0099)	(0.0092)	(0.0095)	(0.0110)	(0.0114)	(0.0200)	(0.0116)
GendNeut	-0.0020	-0.0009	-0.0060	-0.0069	-0.0027	-0.0052	-0.0011	-0.0211	-0.0059
	(0.0068)	(0.0070)	(0.0093)	(0.0100)	(0.0059)	(0.0051)	(0.0117)	(0.0208)	(0.0078)
State Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
State-level Demographic & Policy Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sample Size	622,856	337,482	223,200	69,493	529,648	303,067	176,791	22,012	36,962

Note: Individual demographic controls include age, marital status (when the sample is not restricted by marital status), total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). State-level demographic and policy controls include age composition variables indicating the share of states' populations aged 26 - 40, 41 - 55, 56 - 65, and over 65, the share of the state's population that is black, the natural log of state personal income per-capita, and the state's law regarding abortion access. Standard errors, in parenthesis, are clustered by state.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college (or has at least a bachelor degree in the 1992 to 1999 Surveys).

Table 3.9: Specification (3.1) Results - Full Samples (Males)

	Dependent variable: grad†							
	US Census				Current Population Survey			
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unilateral††	0.0115 (0.0085)	-0.0010 (0.0035)	-0.0038 (0.0039)	-0.0061 (0.0038)	-0.0048 (0.0090)	-0.0154 (0.0067)	-0.0144 (0.0049)	-0.0159 (0.0042)
State Fixed Effects		✓	✓	✓		✓	✓	✓
Year Fixed Effects		✓	✓	✓		✓	✓	✓
Individual Demographic Controls			✓	✓			✓	✓
State-level Demographic & Policy Controls				✓				✓
Sample Size	2,101,351	2,101,351	2,101,351	2,101,351	590,435	590,435	590,435	590,435

Note: Individual demographic controls include age, marital status, total number of children, number of children under the age of 5, and race. State-level demographic and policy controls include age composition variables indicating the share of states' populations aged 26 - 40, 41 - 55, 56 - 65, and over 65, the share of the state's population that is black, the natural log of state personal income per-capita, and the state's law regarding abortion access. Standard errors, in parenthesis, are clustered by state.

† *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college.

†† Unilateral is a binary variable equal to 1 if the state has already adopted unilateral divorce at year t (in the case of the US Census, this takes the value of 1 if the year is 1980 and the state adopted the law between 1970 and 1980).

Table 3.10: Specification (3.1) Results - Samples Restricted by Marital Status and Race (Males)

	Dependent variable: grad[†]							
	US Census				Current Population Survey			
Variable	Married Males (1)	Unmarried Males (2)	Black Males (3)	White Males (4)	Married Males (5)	Unmarried Males (6)	Black Males (7)	White Males (8)
Unilateral [††]	-0.0094 (0.0038)	-0.0006 (0.0044)	-0.0034 (0.0046)	-0.0069 (0.0038)	-0.0187 (0.0055)	-0.0109 (0.0050)	-0.0024 (0.0112)	-0.0151 (0.0043)
State Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓	✓	✓
State-level Demographic & Policy Controls	✓	✓	✓	✓	✓	✓	✓	✓
Sample Size	1,033,707	930,608	238,175	1,805,564	285,541	271,403	53,110	515,272

Note: Individual demographic controls include age, marital status (when the sample is not restricted by marital status), total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). State-level demographic and policy controls include age composition variables indicating the share of states' populations aged 26 - 40, 41 - 55, 56 - 65, and over 65, the share of the state's population that is black, the natural log of state personal income per-capita, and the state's law regarding abortion access. Standard errors, in parenthesis, are clustered by state.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college (or has at least a bachelor degree in the 1992 to 1999 Surveys).

[††] Unilateral is a binary variable equal to 1 if the state has already adopted unilateral divorce at year t.

Table 3.11: Specification (3.1) Results - Samples Further Restricted by Marital Status and Race (Males)

	Dependent variable: grad†							
	US Census				Current Population Survey			
Variable	White Married Males (1)	White Unmarried Males (2)	Black Married Males (3)	Black Unmarried Males (4)	White Married Males (5)	White Unmarried Males (6)	Black Married Males (7)	Black Unmarried Males (8)
Unilateral ††	-0.0080 (0.0036)	-0.0036 (0.0052)	-0.0052 (0.0054)	-0.0047 (0.0045)	-0.0172 (0.0058)	-0.0119 (0.0057)	-0.0058 (0.0175)	0.0010 (0.0099)
State Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓	✓	✓
State-level Demographic & Policy Controls	✓	✓	✓	✓	✓	✓	✓	✓
Sample Size	920,582	772,993	87,403	128,883	257,717	228,824	18,997	30,264

Note: Individual demographic controls include age, marital status (when the sample is not restricted by marital status), total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). State-level demographic and policy controls include age composition variables indicating the share of states' populations aged 26 - 40, 41 - 55, 56 - 65, and over 65, the share of the state's population that is black, the natural log of state personal income per-capita, and the state's law regarding abortion access. Standard errors, in parenthesis, are clustered by state.

† *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college (or has at least a bachelor degree in the 1992 to 1999 Surveys).

†† Unilateral is a binary variable equal to 1 if the state has already adopted unilateral divorce at year *t*.

Table 3.12: Specification (3.2) Results (Males)

Variable	Full Sample (1)	Married Males (2)	Unmarried Males (3)	Black Males (4)	White Males (5)	White Married Males (6)	White Unmarried Males (7)	Black Married Males (8)	Black Unmarried Males (9)
	Dependent variable: grad†								
Unilateral×CommProp	-0.0177	-0.0288	-0.0067	0.0034	-0.0188	-0.0307	-0.0086	0.0049	0.0004
	(0.0043)	(0.0062)	(0.0066)	(0.0171)	(0.0049)	(0.0071)	(0.0075)	(0.0221)	(0.0168)
Unilateral×Title	-0.0027	-0.0058	0.0063	-0.0182	0.0009	-0.0026	0.0081	-0.0175	-0.0116
	(0.0054)	(0.0075)	(0.0073)	(0.0151)	(0.0071)	(0.0090)	(0.0090)	(0.0232)	(0.0112)
Unilateral×EquitDistr	-0.0077	-0.0042	-0.0105	-0.0065	-0.0053	-0.0007	-0.0101	-0.0131	-0.0016
	(0.0046)	(0.0055)	(0.0065)	(0.0109)	(0.0049)	(0.0056)	(0.0076)	(0.0203)	(0.0096)
Commprop	-0.0043	0.0035	-0.0150	-0.0194	-0.0036	0.0054	-0.0152	-0.0373	-0.0058
	(0.0036)	(0.0035)	(0.0047)	(0.0050)	(0.0035)	(0.0036)	(0.0047)	(0.0116)	(0.0053)
EquitDistr	0.0106	0.0092	0.0110	-0.0075	0.0115	0.0094	0.0134	-0.0076	-0.0117
	(0.0037)	(0.0050)	(0.0043)	(0.0057)	(0.0037)	(0.0051)	(0.0045)	(0.0108)	(0.0069)
State Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
State-level Demographic & Policy Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sample Size	590,435	285,541	271,403	53,110	515,272	257,717	228,824	18,997	30,264

Note: Individual demographic controls include age, marital status (when the sample is not restricted by marital status), total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). State-level demographic and policy controls include age composition variables indicating the share of states' populations aged 26 - 40, 41 - 55, 56 - 65, and over 65, the share of the state's population that is black, the natural log of state personal income per-capita, and the state's law regarding abortion access. Standard errors, in parenthesis, are clustered by state.

† *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college (or has at least a bachelor degree in the 1992 to 1999 Surveys).

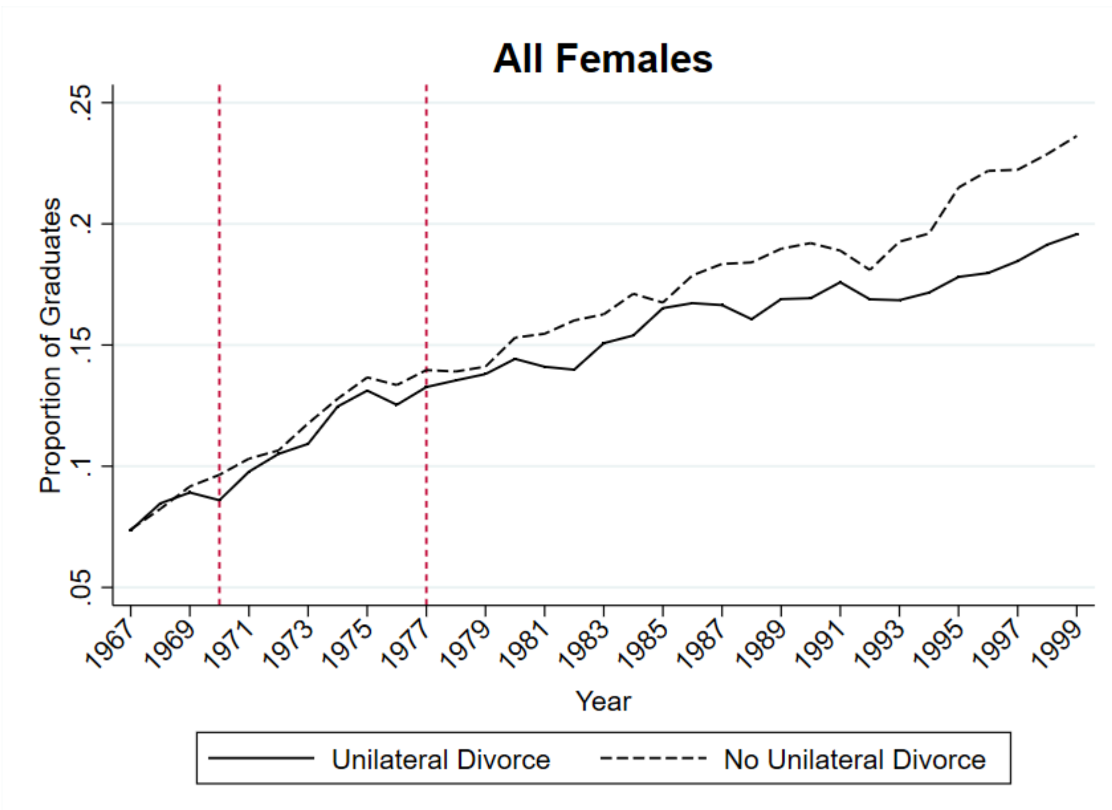
Table 3.13: Specification (3.3) Results (Males)

Variable	Full Sample (1)	Married Females (2)	Unmarried Females (3)	Black Females (4)	White Females (5)	White Married Females (6)	White Unmarried Females (7)	Black Married Females (8)	Black Unmarried Females (9)
	Dependent variable: grad†								
Unilateral×GendNeut	-0.0089	-0.0105	-0.0021	-0.0036	-0.0096	-0.0087	-0.0043	-0.0065	0.0013
	(0.0062)	(0.0076)	(0.0084)	(0.0133)	(0.0067)	(0.0067)	(0.0104)	(0.0259)	(0.0128)
Unilateral×NonGendNeut	-0.0058	-0.0107	-0.0016	-0.0077	-0.0055	-0.0087	-0.0054	-0.0261	0.0165
	(0.0058)	(0.0085)	(0.0069)	(0.0166)	(0.0062)	(0.0085)	(0.0078)	(0.0221)	(0.0241)
GendNeut	-0.0020	-0.0006	-0.0027	-0.0106	0.0007	0.0023	-0.0027	-0.0270*	0.0023
	(0.0043)	(0.0046)	(0.0050)	(0.0071)	(0.0044)	(0.0039)	(0.0055)	(0.0154)	(0.0050)
State Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
State-level Demographic & Policy Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sample Size	580,248	279,006	267,958	52,090	506,229	251,827	225,826	18,415	29,878

Note: Individual demographic controls include age, marital status (when the sample is not restricted by marital status), total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). State-level demographic and policy controls include age composition variables indicating the share of states' populations aged 26 - 40, 41 - 55, 56 - 65, and over 65, the share of the state's population that is black, the natural log of state personal income per-capita, and the state's law regarding abortion access. Standard errors, in parenthesis, are clustered by state.

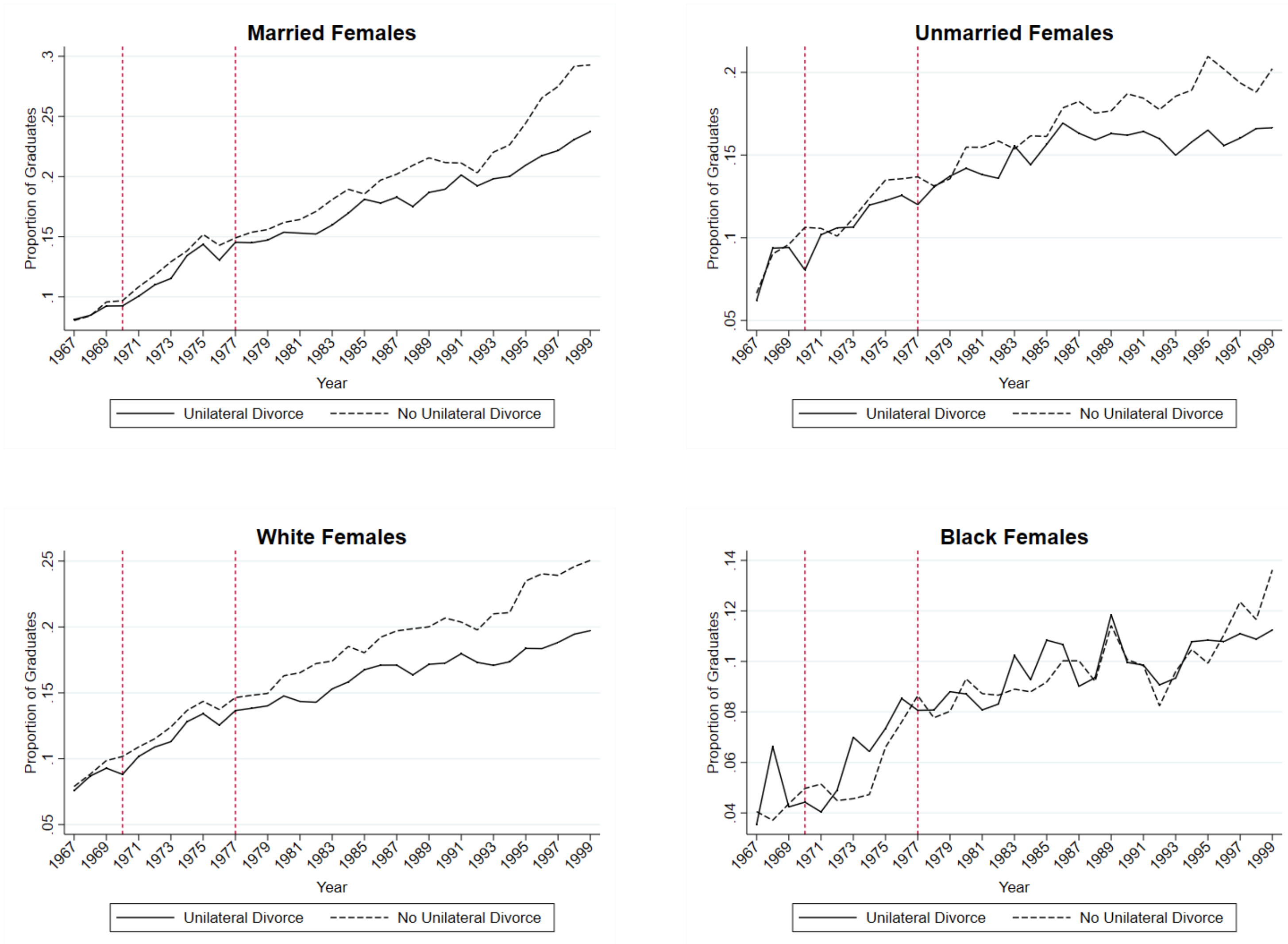
† *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college (or has at least a bachelor degree in the 1992 to 1999 Surveys).

Figure 3.1: Proportion of Female Graduates Over Time in Unilateral and Non-Unilateral States



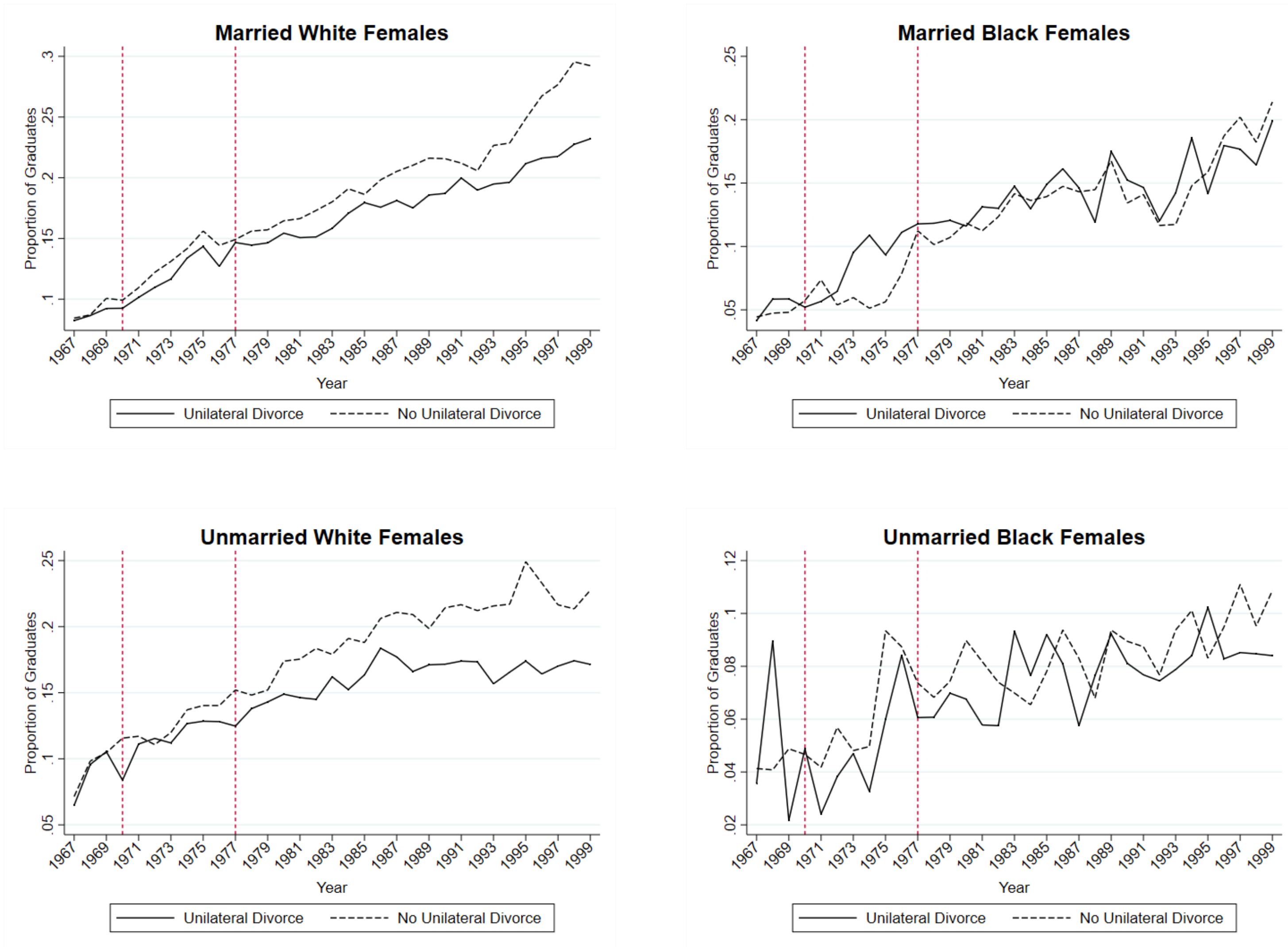
Note: The dashed red vertical lines indicate the period over which the majority of states adopted unilateral divorce.

Figure 3.2: Proportion of Female Graduates Over Time in Unilateral and Non-Unilateral States by Marital Status and Race



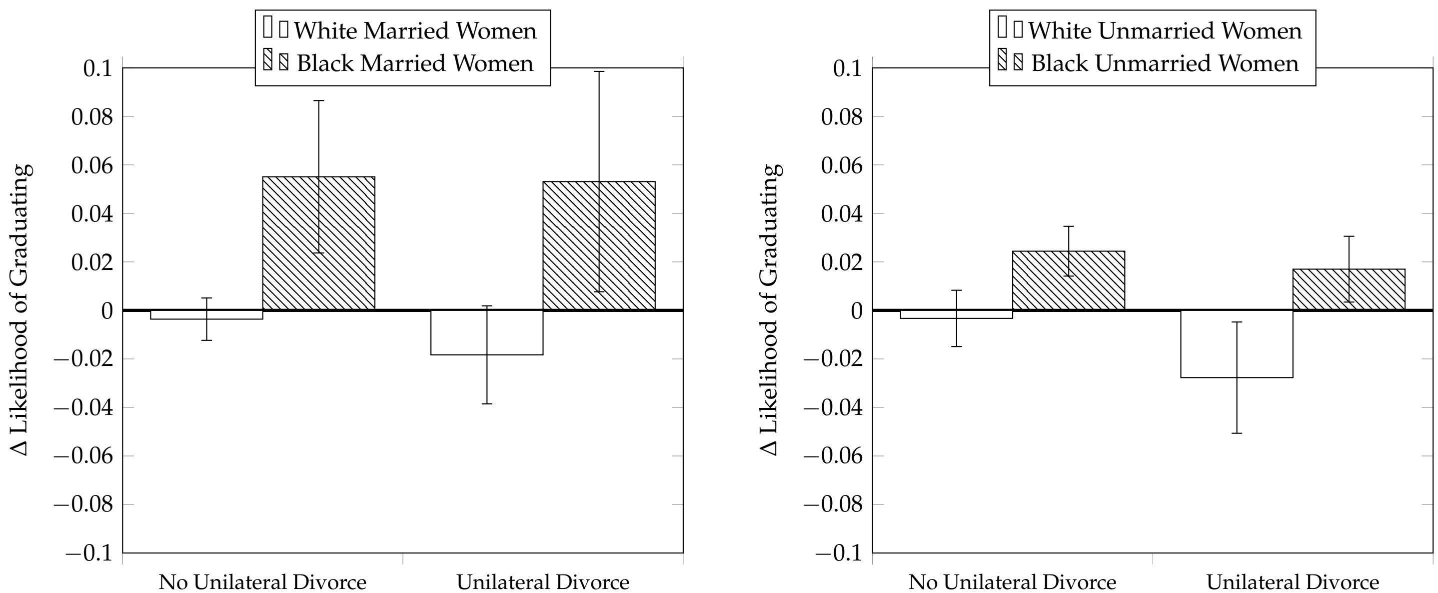
Note: The dashed red vertical lines indicate the period over which the majority of states adopted unilateral divorce.

Figure 3.3: Proportion of Female Graduates Over Time in Unilateral and Non-Unilateral States by Marital Status and Race



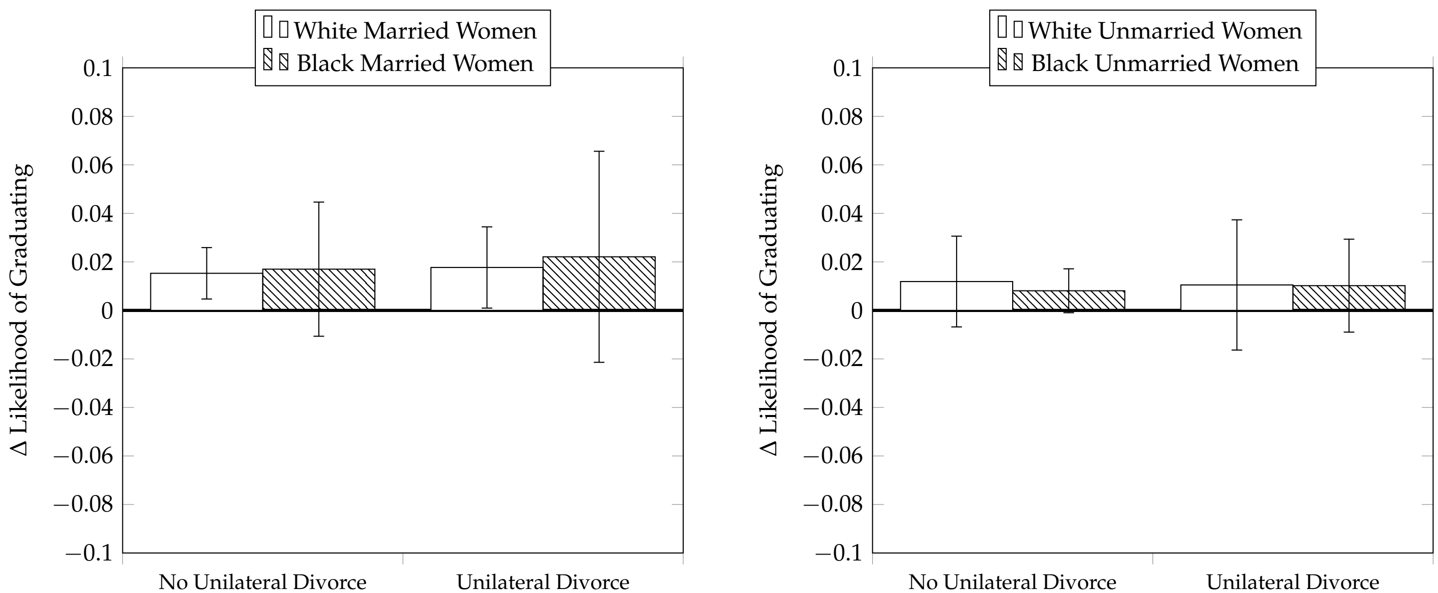
Note: The dashed red vertical lines indicate the period over which the majority of states adopted unilateral divorce.

Figure 3.4: Effect of Unilateral Divorce on Likelihood of Graduating for Females in Community Property States Relative to Females in Title Based States by Race and Marital Status



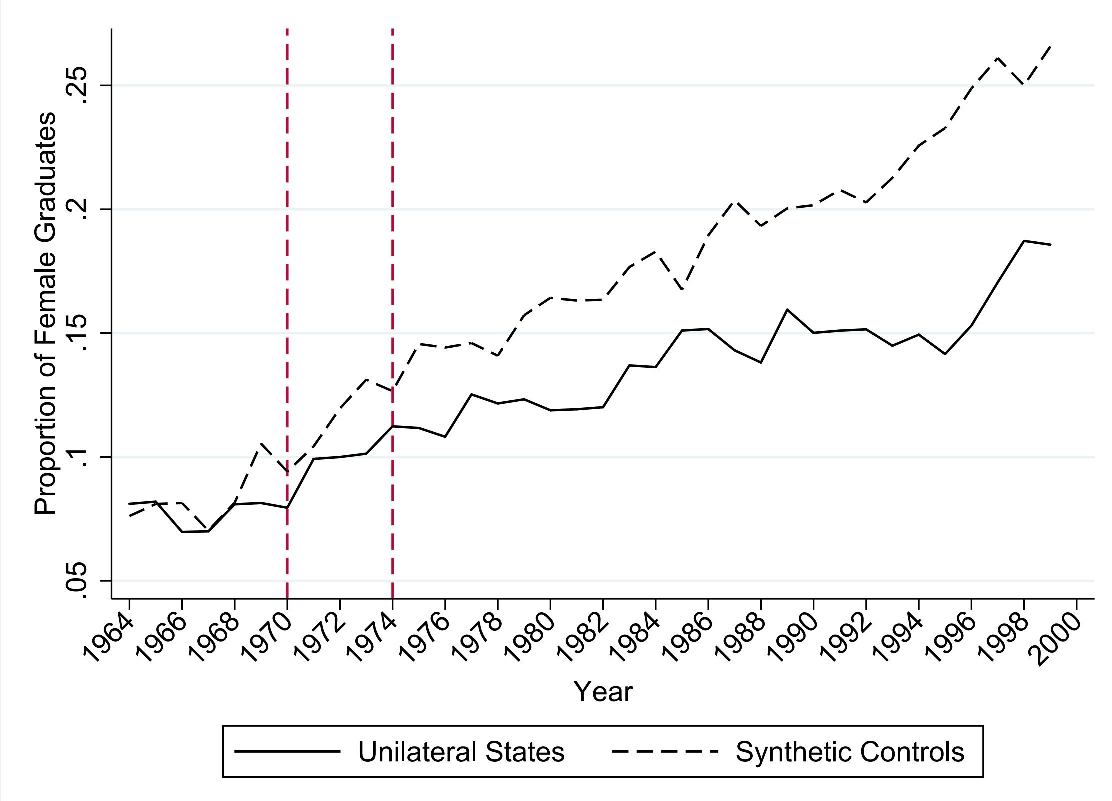
Note: The left two bars in each figure capture the effect on college attainment in mutual consent divorce states of having a community property regime relative to having a title-based regime for white and black married and unmarried women (these are estimates of β_4 in equation (3.2)). The right two bars in each figure capture the effect on college attainment in unilateral divorce states of having a community property regime relative to having a title-based regime for white and black married and unmarried women (these are estimates of $\beta_1 + \beta_4$ in equation (3.2)). The stems represent 95% confidence intervals for each estimate.

Figure 3.5: Effect of Unilateral Divorce on Likelihood of Graduating for Females in Equitable Distribution States Relative to Females in Title Based States by Race and Marital Status



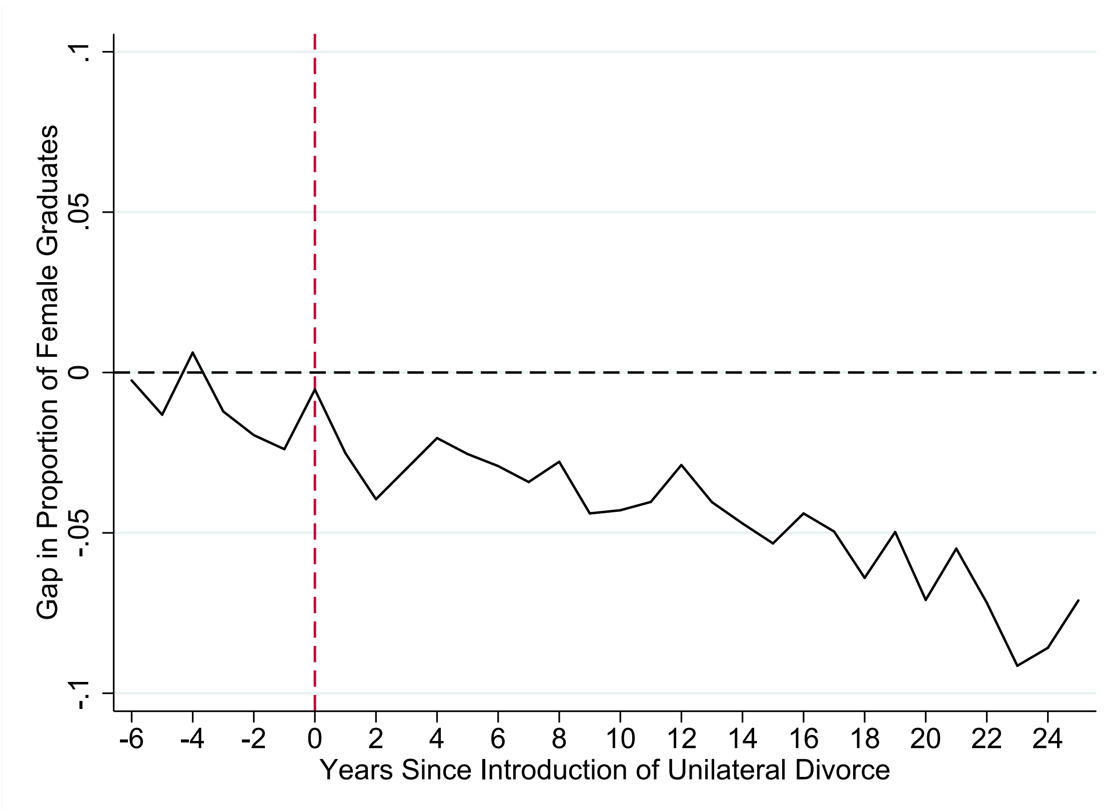
Note: The left two bars in each figure capture the effect on college attainment in mutual consent divorce states of having an equitable distribution regime relative to having a title-based regime for white and black married and unmarried women (these are estimates of β_5 in equation (3.2)). The right two bars in each figure capture the effect on college attainment in unilateral divorce states of having an equitable distribution regime relative to having a title-based regime for white and black married and unmarried women (these are estimates of $\beta_3 + \beta_5$ in equation (3.2)). The stems represent 95% confidence intervals for each estimate.

Figure 3.6: Averages by Year in the Proportion of Female Graduates Over Time in Unilateral States and their Synthetic Controls



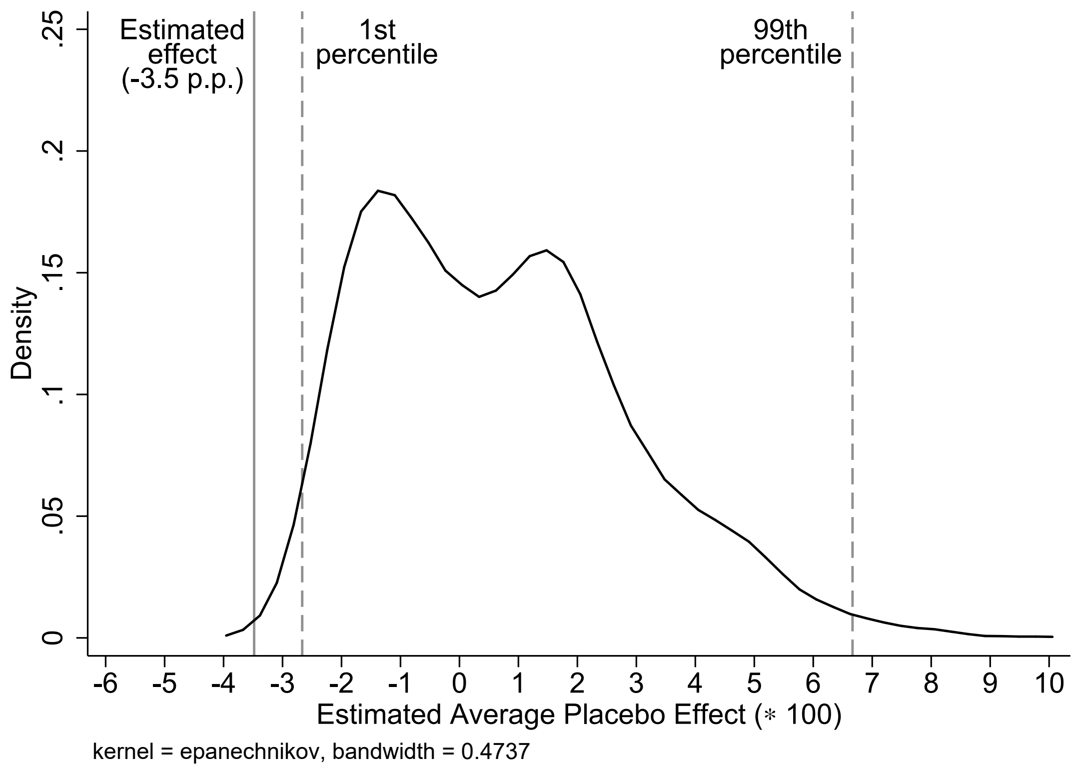
Note: The dashed red vertical lines indicate the period over which states in our sub-sample adopted unilateral divorce.

Figure 3.7: Differences in the Average Proportion of Female Graduates Between Unilateral States and their Synthetic Controls by Year Since Reform



Note: The dashed red vertical line at 0 indicates the year in which states in our sub-sample adopted unilateral divorce.

Figure 3.8: Distribution of Estimated Average Placebo Effects (Females)

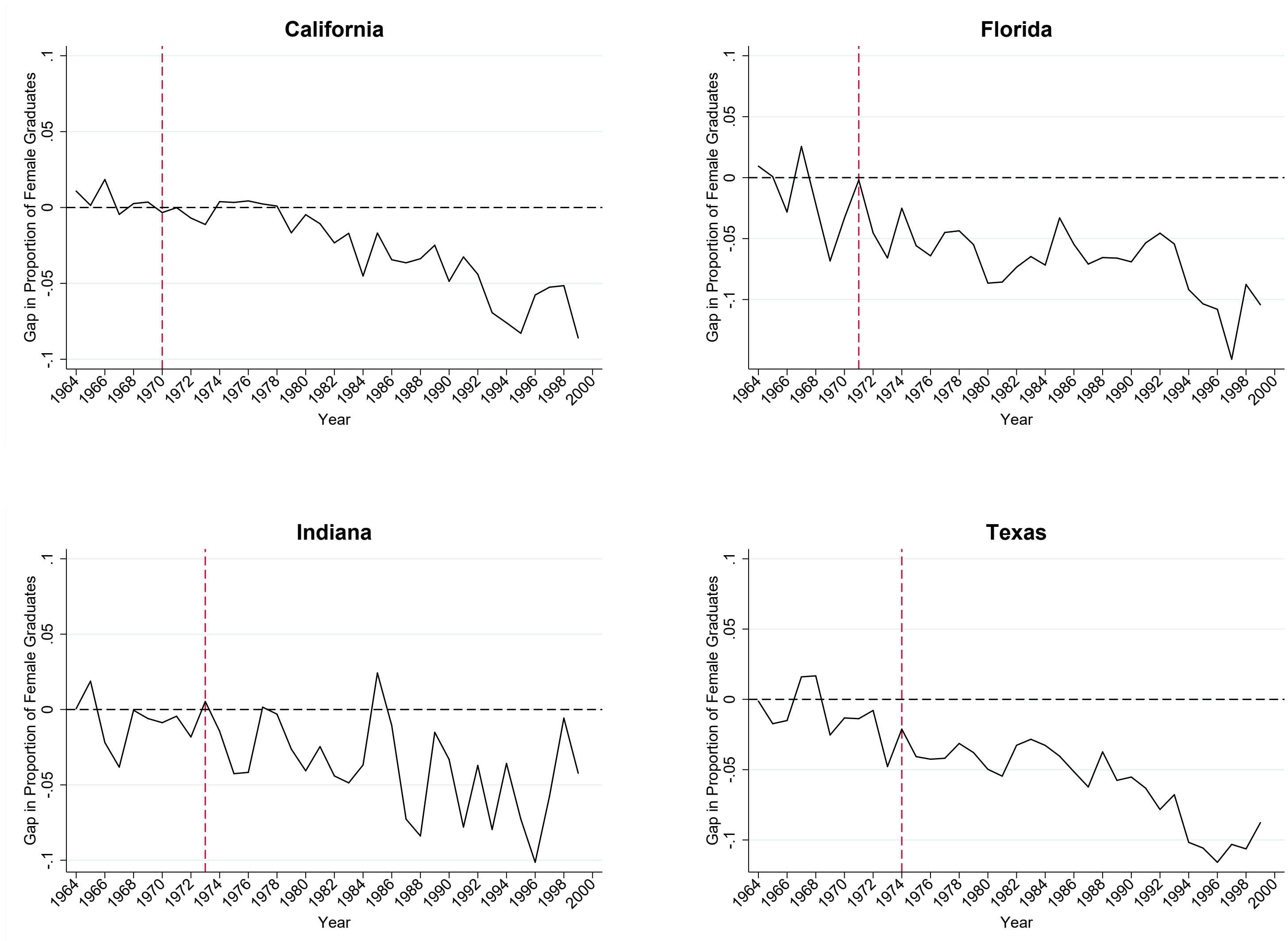


Note: This is a kernel density plot of the 1,296 estimated average placebo effects. The dashed gray vertical lines indicate the 1st and 99th percentiles in the distribution of estimated average placebo effects. The solid gray vertical line indicates our estimated average treatment effect of -3.5 p.p.

Figure 3.9: Proportion of Female Graduates Over Time in Unilateral States and their Synthetic Controls

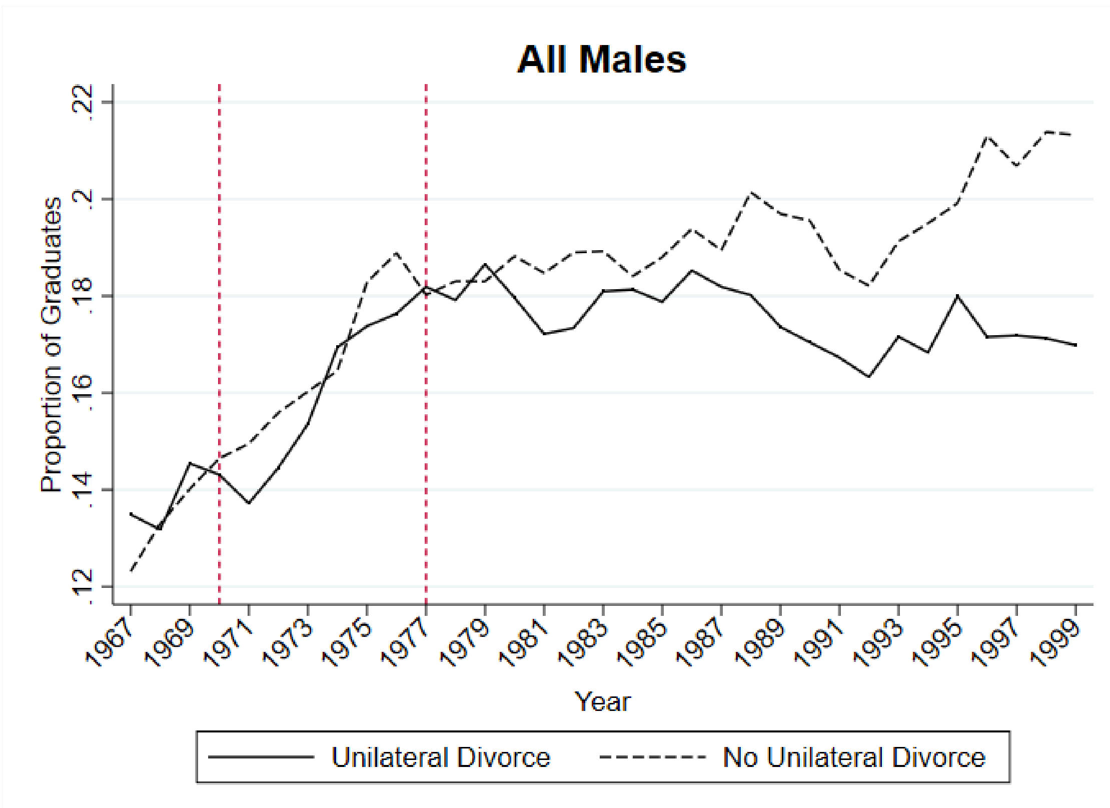
Note: The dashed red vertical lines indicate the year in which each respective state adopted unilateral divorce.

Figure 3.10: Difference Between the Proportion of Female Graduates in Unilateral States and their Synthetic Controls Over Time



Note: The dashed red vertical lines indicate the year in which each respective state adopted unilateral divorce.

Figure 3.11: Proportion of Male Graduates Over Time in Unilateral and Non-Unilateral States

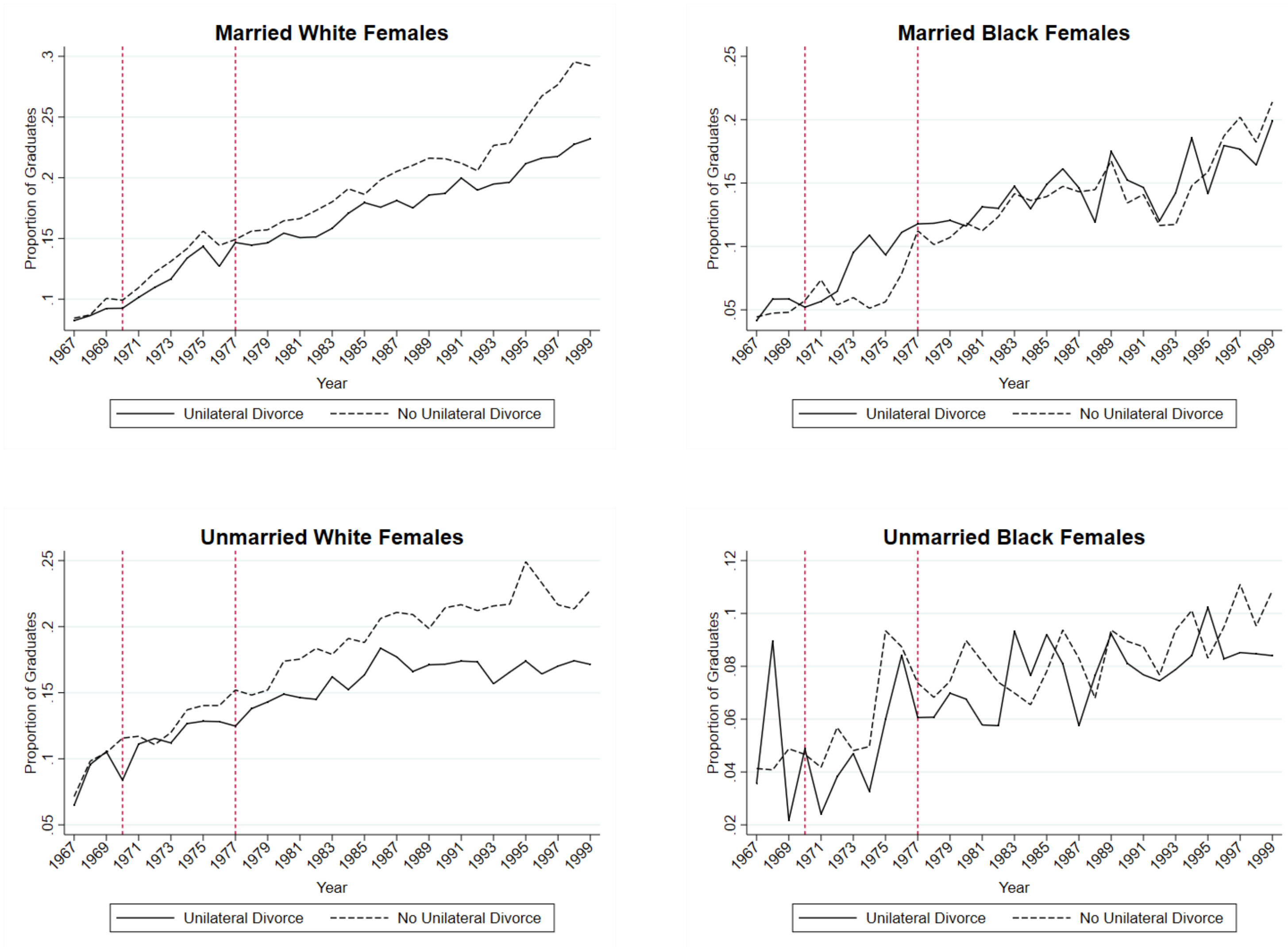


Note: The dashed red vertical lines indicate the period over which states adopted unilateral divorce.

Figure 3.12: Proportion of Male Graduates Over Time in Unilateral and Non-Unilateral States by Marital Status and Race

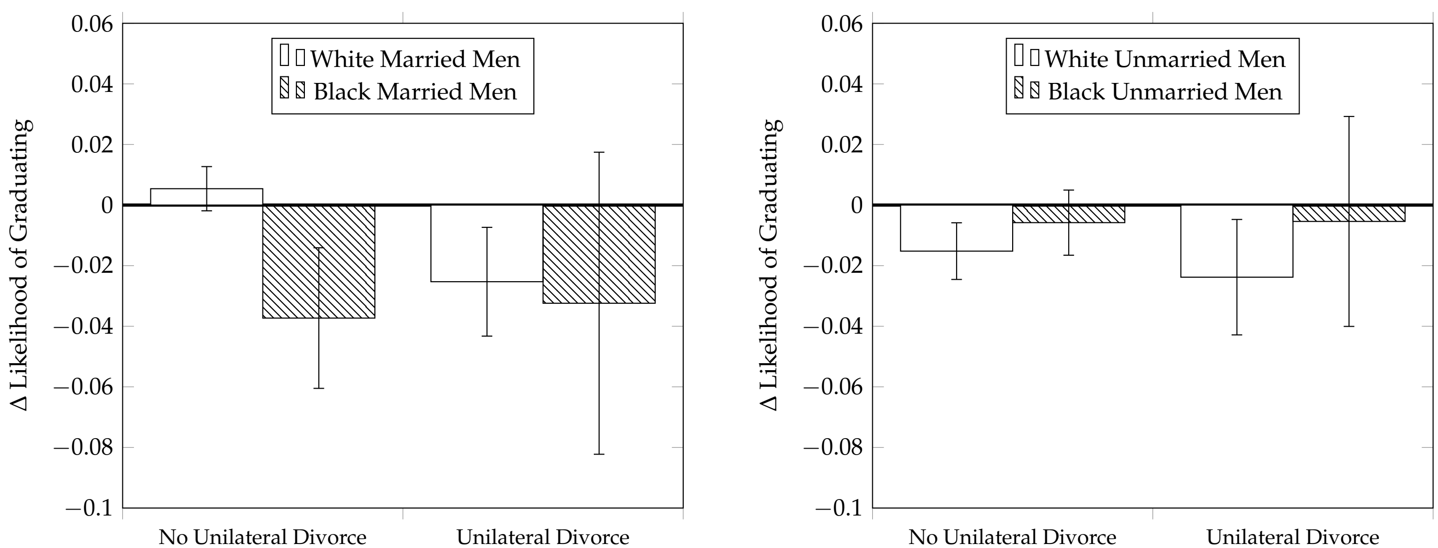
Note: The dashed red vertical lines indicate the period over which states adopted unilateral divorce.

Figure 3.13: Proportion of Male Graduates Over Time in Unilateral and Non-Unilateral States by Marital Status and Race



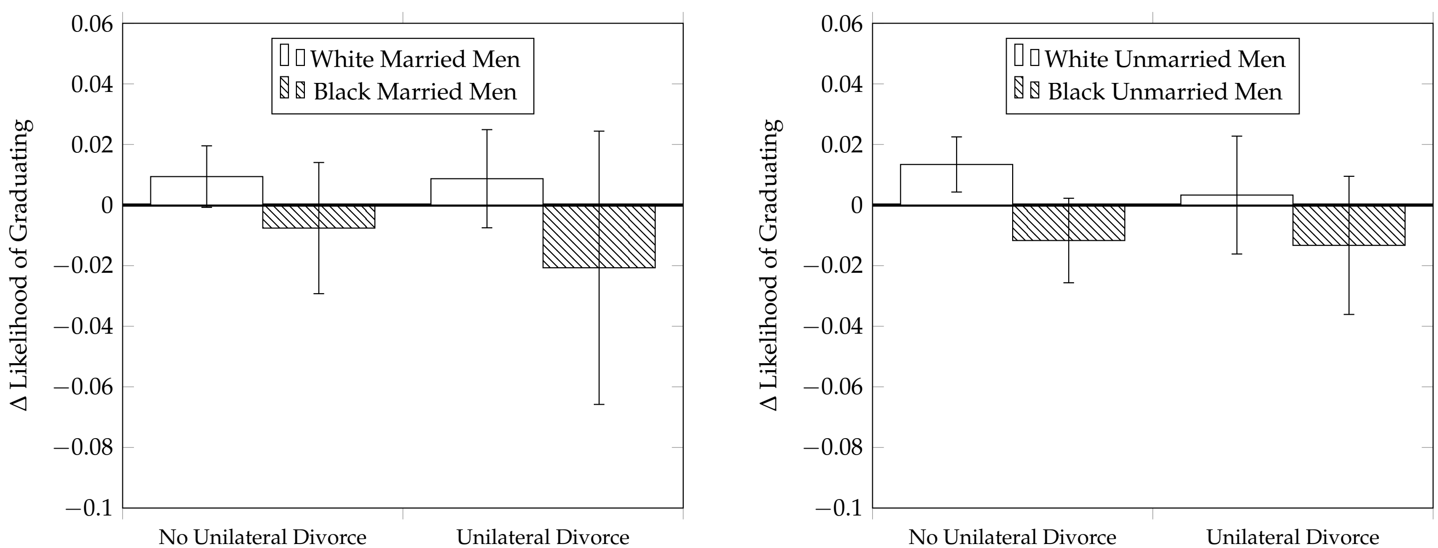
Note: The dashed red vertical lines indicate the period over which states adopted unilateral divorce.

Figure 3.14: Effect of Unilateral Divorce on Likelihood of Graduating for Males in Community Property States Relative to Males in Title Based States by Race and Marital Status



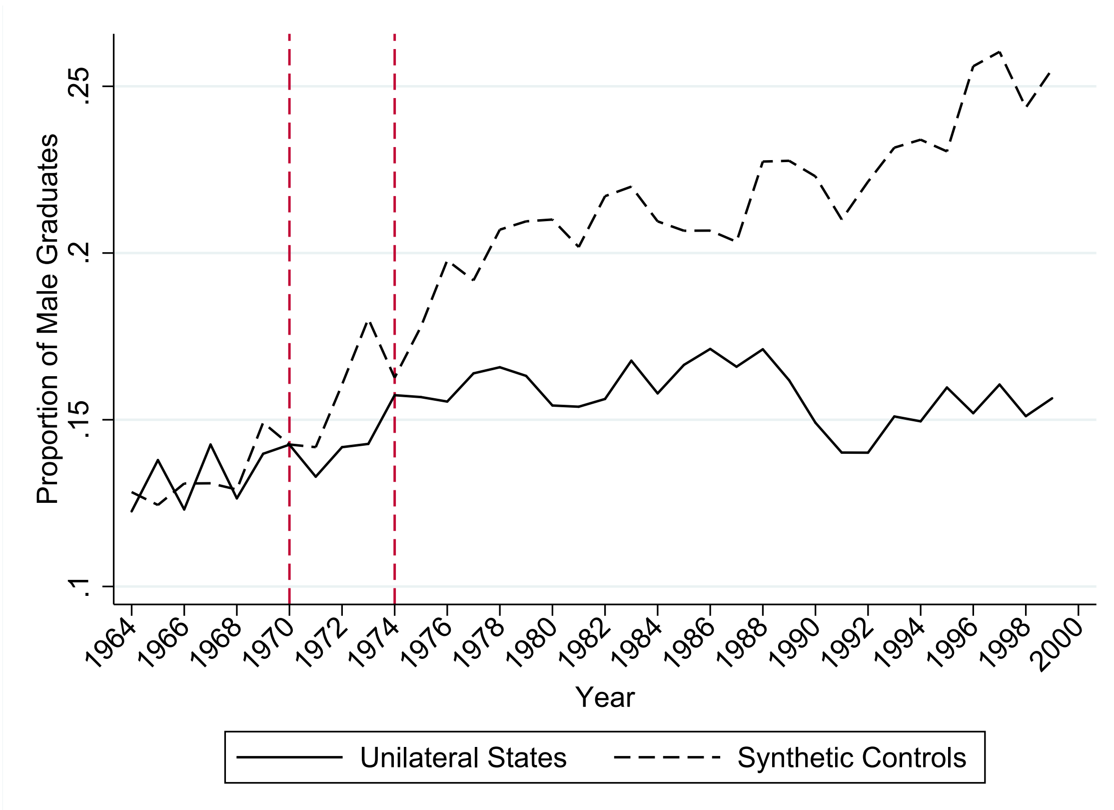
Note: The left two bars in each figure capture the effect on college attainment in mutual consent divorce states of having a community property regime relative to having a title-based regime for white and black married and unmarried men (these are estimates of β_4 in equation (3.2)). The right two bars in each figure capture the effect on college attainment in unilateral divorce states of having a community property regime relative to having a title-based regime for white and black married and unmarried men (these are estimates of $\beta_1 + \beta_4$ in equation (3.2)). The stems represent 95% confidence intervals for each estimate.

Figure 3.15: Effect of Unilateral Divorce on Likelihood of Graduating for Males in Equitable Distribution States Relative to Males in Title Based States by Race and Marital Status



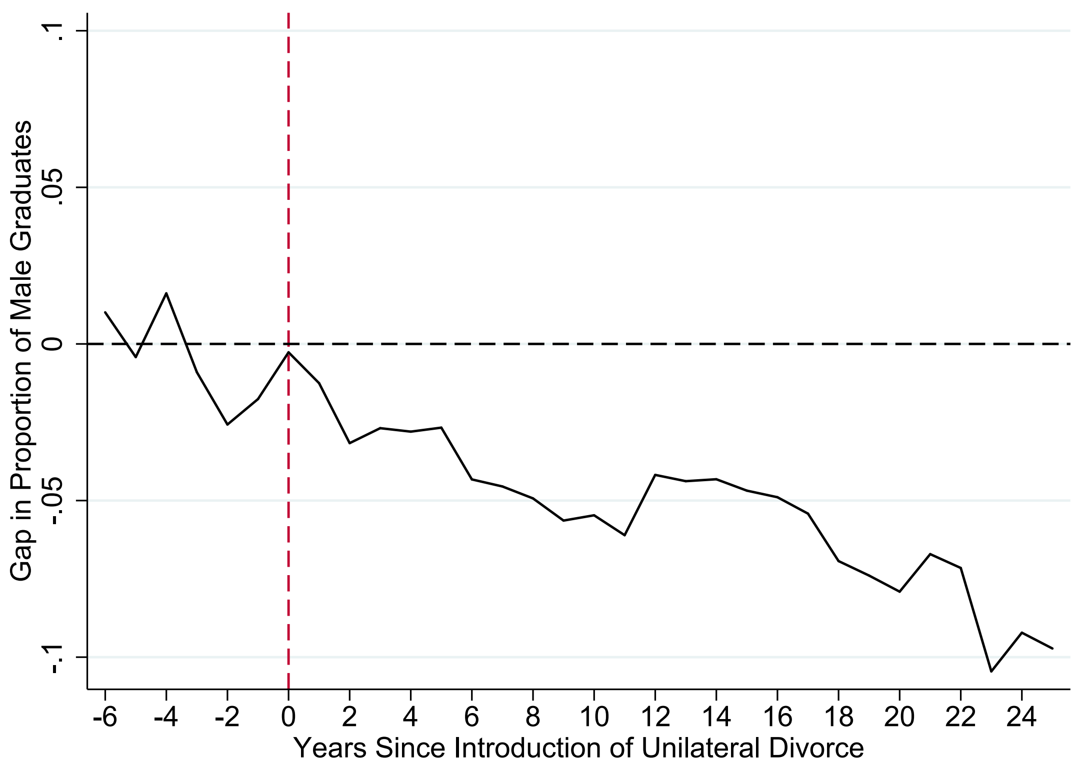
Note: The left two bars in each figure capture the effect on college attainment in mutual consent divorce states of having an equitable distribution regime relative to having a title-based regime for white and black married and unmarried men (these are estimates of β_5 in equation (3.2)). The right two bars in each figure capture the effect on college attainment in unilateral divorce states of having an equitable distribution regime relative to having a title-based regime for white and black married and unmarried men (these are estimates of $\beta_3 + \beta_5$ in equation (3.2)). The stems represent 95% confidence intervals for each estimate.

Figure 3.16: Averages by Year in the Proportion of Male Graduates Over Time in Unilateral States and their Synthetic Controls



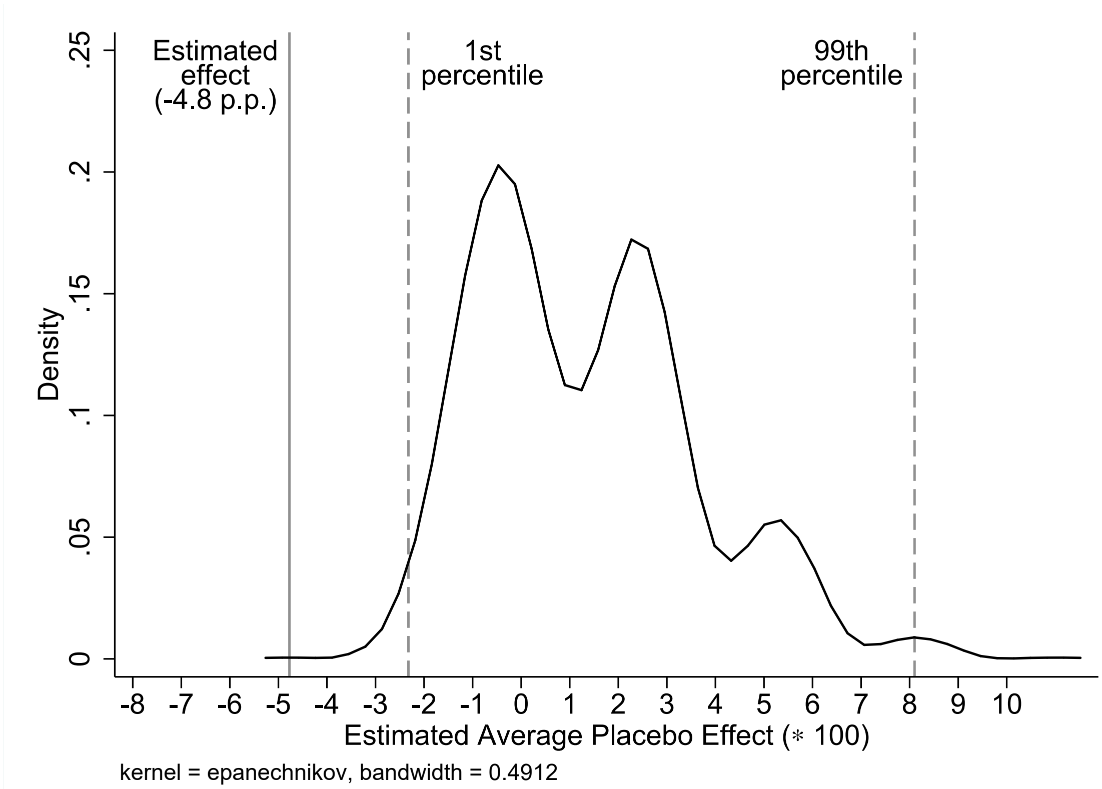
Note: The dashed red vertical lines indicate the period over which states in our sub-sample adopted unilateral divorce.

Figure 3.17: Differences in the Average Proportion of Male Graduates Between Unilateral States and their Synthetic Controls by Year Since Reform



Note: The dashed red vertical line at 0 indicates the year in which states in our sub-sample adopted unilateral divorce.

Figure 3.18: Distribution of Estimated Average Placebo Effects (Males)

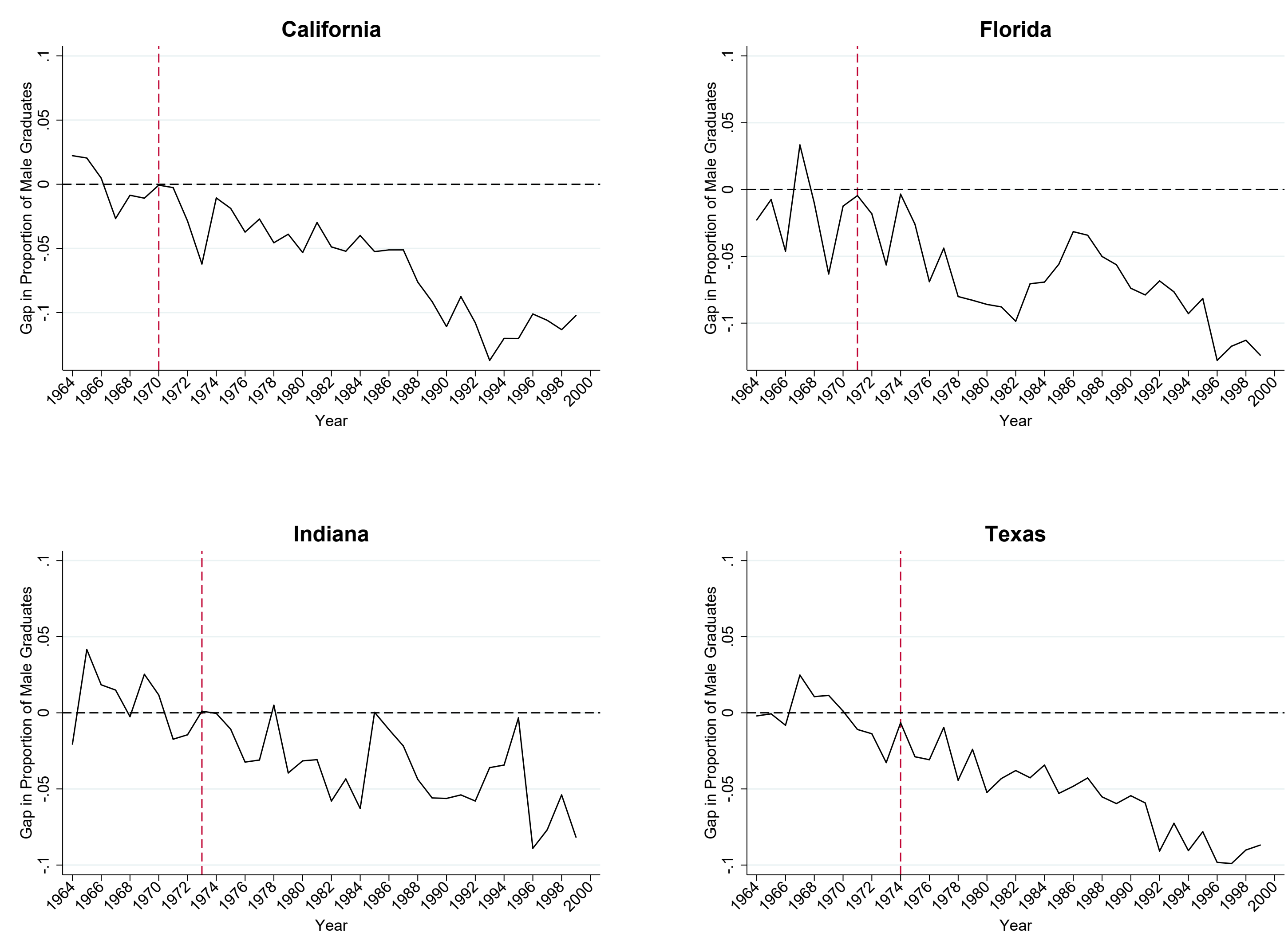


Note: This is a kernel density plot of the 1,296 estimated average placebo effects. The dashed gray vertical lines indicate the 1st and 99th percentiles in the distribution of estimated average placebo effects. The solid gray vertical line indicates our estimated average treatment effect of -4.8 p.p.

Figure 3.19: Proportion of Male Graduates Over Time in Unilateral States and their Synthetic Controls

Note: The dashed red vertical lines indicate the year in which each respective state adopted unilateral divorce.

Figure 3.20: Difference Between the Proportion of Male Graduates in Unilateral States and their Synthetic Controls Over Time



Note: The dashed red vertical lines indicate the year in which each respective state adopted unilateral divorce.

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